CESifo Working Papers

www.cesifo.org/wp

An Offer You Can't Refuse?

Incentives Change How We Inform Ourselves and What We Believe

Sandro Ambuehl

CESIFO WORKING PAPER NO. 6296
CATEGORY 13: BEHAVIOURAL ECONOMICS
ORIGINAL VERSION: JANUARY 2017
THIS VERSION: OCTOBER 2017

ISSN 2364-1428

CESifo
Center for Economic Studies & Ifo Institute

CESifo *Working Paper No. 6296*

An Offer You Can't Refuse?
Incentives Change How We Inform Ourselves and What We Believe

Abstract

Economists often espouse incentives, arguing that expanding choice sets cannot lower welfare. Yet, laws worldwide restrict incentives for many transactions, partly due to an untested concern that incentives cause poor decisions. I show experimentally that incentives skew information gathering and beliefs about what a transaction entails in a way that causally influences the participation decision, as policy makers suspected. A model of costly information acquisition shows this behavior is consistent with rationality, and thus unconcerning from an ex ante welfare economic perspective, but demands consideration under reasonable alternatives. The mechanisms apply in any situation where incentives interact with information acquisition.

JEL-Codes: D030, D040, D840.

Keywords: incentives, repugnant transactions, information acquisition, inattention, experiment.

Sandro Ambuehl
UTSC Department of Management &
Rotman School of Management
University of Toronto
105 St George Street
Canada – Toronto, ON M5S 3E6
sandro.ambuehl@utoronto.ca

October 26, 2017
This is a thoroughly revised version of my job market paper that I wrote whilst at Stanford University. I am deeply indebted to my advisors Muriel Niederle, B. Douglas Bernheim, and Alvin E. Roth. I am very grateful to Debra Satz for many discussions about philosophers' work on the ethics of incentives, and to P.J. Healy and Yan Chen for providing experimental laboratory resources. This work also benefitted from countless helpful comments by seminar participants at CERGE-EI, the University of Melbourne, the University of Toronto, the Wharton School at the University of Pennsylvania, Carnegie Mellon University, the University of California in Los Angeles, the University of Cologne, the University of Munich, the University of Groningen, the Berlin Social Science Centre, Stanford University, as well as at the Behavioral Economics Annual Meetings, the CESifo area conference in behavioral economics, the Congress of the European Economic Association, the Bay Area Behavioral and Experimental Economics Workshop, the Centre for Ethics at the University of Toronto, and the Workshop on Moral Reasoning in Economics at the briq Institute. This work has been approved by the Stanford IRB in protocols 29615 (online experiments) and 34001 (laboratory experiments). Funding from the Stanford Department of Economics is gratefully acknowledged.

Offers of large sums of money . . . could lead some prospective participants to enroll ... when it might be against their better judgment and when otherwise they would not do so.

- National Bioethics Advisory Commission (2001)

1 Introduction

Many policies incentivize people to participate in transactions about which they know little. In these cases people typically acquire information before they are willing to make a decision. For certain such transactions, including organ donation, gestational surrogacy, egg donation, or medical trial participation, countries around the world tightly restrict incentives, or outlaw them altogether, despite the potentially enormous welfare costs of such laws.[1] One important reason for these restrictions (among others) is the fear that incentives would interfere with sound decision making. According to the Stanford Encyclopedia of Philosophy (2017), the influential concept of *undue inducements* entails "that something is being offered that is alluring to the point that it clouds rational judgment ... Attention is fixated on the benefit, disallowing proper consideration of the risks". Similarly, the American Society for Reproductive Medicine (2007) urges that "Payments to women providing oocytes should be ... not so substantial that they ... lead donors to discount risks," and speculates that "the higher the payment, the greater the possibility that women will discount risks."[2] These concerns are foreign to many economists, on two grounds. First, economists are keenly aware that one cannot, in expectation, make a rational decision maker worse off by offering higher incentives for a transaction. Second, the concern that incentives somehow warp individuals' perception of the transaction appears at odds with the standard rational agent model. Thus, many economists believe that ethical concerns about incentives are at least partly due to confusion about elementary economics (Becker and Elias, 2007). However, an investigation into the effects of incentives on the nature and quality of decision making for transactions with complex and potentially hard-to-understand consequences has been lacking.

In this paper, I study how incentives affect the quality of decisions in contexts where information about the transaction is costly. In two behavioral experiments, I find that higher

[1] For instance, Held et al. (2016) estimate that the ban on incentives for kidney donation is responsible for the premature death of up to ten thousand Americans on the waiting list each year. The same authors estimate that restrictions on incentives for kidney donation cost taxpayers $12 billion annually, due to the expensive nature of current treatments for kidney disease relative to the estimated cost of a donor kidney.

[2] See also the quote by the National Bioethics Advisory Commission (2001) above, as well as Kanbur (2004), Satz (2010), Grant (2011), and Sandel (2012). Satz explicitly mentions "a paternalistic concern that sellers would actually be harmed by the sale of their organs, but that ... they would sell their organs if it were legal."

incentives skew subjects' information acquisition towards encouraging sources and away from discouraging ones. As a result, they shape subjects' beliefs about the deliverable, which, in turn, causally influence the ensuing participation decision. I demonstrate that this mechanism applies not only to experimental subjects, but also in a model of rational Bayesian agents. That model both helps clarify the welfare implications of the effects I document, and shows which empirical findings are, and are not, evidence for poor decision making. Most fundamentally, I show that by shaping people's beliefs about their choice options, incentives genuinely persuade.[3]

This result has implications in any situation in which information acquisition and incentives interact, encompassing fields as diverse as finance, marketing, and political economy (detailed in Section 6). In the domain of ethical concerns with incentives, this result shows that incentives can make people appear, for example, as though they "discount risks", even if they are entirely rational. For social planners concerned with the *expected* welfare of entirely rational individuals, incentives lead to ostensibly concerning behavior, that, upon closer inspection, is in fact no cause for alarm. In case of imperfectly rational individuals, or for planners whose welfare criterion does not solely depend on the sum of expected utilities (for instance, because it accounts for *ex post* inequality, see Cappelen et al. (2013); Andreoni et al. (2015)), objections may indeed be justified in specific cases.

This paper consists of three main parts: two experiments and a simple theoretical model. The first experiment emphasizes ecological validity and thus uses a transaction about which subjects have homegrown preferences and prior beliefs. The second experiment is complementary to the first, and is designed to precisely determine the behavioral mechanisms in a more stylized setting.

For the first experiment, I select a transaction that is similar, in a precisely defined fashion, to those for which incentives are restricted by law. Specifically, it needs to be one that subjects have not previously considered, that is visceral, and that presents an opportunity to selectively access multifaceted information. I do not attempt to capture any other aspect of the legally-restricted transactions. Indeed, it is unnecessary to do so, as long as the transaction leads to the type of behavior the ethics literature is concerned about, and thus enables an investigation of that behavior. Because the transaction also needs to be safe and legal, I have experimental participants decide whether to ingest whole insects in exchange for money. Some subjects are offered \$30 for eating an insect while others are offered \$3. Given the offers, but before making a decision, subjects then choose between watching a video with the title "Why you may want to eat insects" and one with the title "Why you may not want to eat insects". Once they

[3] In the language of the ethics literature, I show that incentives lead to *self-persuasion*. The latter is defined as cases in which "an influence agent's intervention does provoke ... self-generated reasons [that] succeed in persuading the person to adopt precisely what the influence agent advocates" (Faden and Beauchamp, 1986).

are done watching the video they have selected, they decide whether to consume the animals in exchange for the promised incentive. I document that the higher incentive induces more demand for the encouraging video and less demand for the discouraging one. Using a control condition, I show that this differential information acquisition causally skews subjects' beliefs about the disutility of eating insects, the distribution of reservation prices, and that it sways the participation decision subjects ultimately make.

At first glance, these results appear to confirm the worries of the ethics literature. The model, based on costly information acquisition, however, shows that such behavior is consistent with rational Bayesian decision making.[4] Intuitively, the reason is the following. When information is costly, a decision maker will often find it optimal to reach a decision based on incomplete information. This exposes her to two possible types of error: A *false positive* occurs if she participates when she would have abstained with full information. On the other hand, a *false negative* occurs if she abstains when she would have participated with full information. The incentive for participation affects information acquisition because an optimizing decision maker will direct attention so as to make more expensive mistakes less often. With a low incentive, one has little to gain from participation but potentially much to lose, so that false positives are the expensive type of mistake. In this case, the decision maker will focus on information she expects to effectively prevent mistaken participation. Hence, she will demand information she expects will likely decrease her beliefs that participation is the right choice—information that is *discouraging*. By contrast, a sufficiently high incentive makes false negatives the more expensive type of mistake. These would cause her to mistakenly forego that large payment. In this case, the decision maker will focus on information that she expects to effectively prevent mistaken abstention. Hence, she will demand information that will likely increase her beliefs that participation is the right choice—information that is encouraging. Hence, even a rational Bayesian may look as though she were trying to convince herself to participate when incentives are high. Generally, this mechanism applies under two conditions: First, the informational environment needs to be sufficiently rich to allow the decision maker to trade off false positives and false negatives. Second, the information costs must be of a similar order of magnitude as the stakes of the decision. Hence, the mechanism applies both in the limited-stakes experiments in this paper, and in highly consequential decisions, such as organ donation, for which fully comprehensive information is extremely expensive to acquire. (To illustrate, most people do not acquire a doctorate in nephrology before deciding whether to donate a kidney.)

[4] Bayesian inference can also explain seemingly irrational behavior in other domains. For instance, Benoît and Dubra (2011) show that it is consistent with Bayesian rationality for 60% of automobilists to consider themselves above-average drivers.

The welfare implications of these findings depend on both the planner's welfare objective, and the extent to which empirical behavior is rational. Thus, I employ a second experiment to study choice in a setting sufficiently stylized to allow for an explicit test of rationality. I first confirm that the observed behavior is consistent with the directional predictions of the theory and conceptually replicates the findings of the first experiment. I then compare objective posterior beliefs with elicited ones and find that they align closely. While this indicates a substantial degree of rationality, the deviations I do observe are those that may be concerning to ethicists and policy makers—higher incentives make subjects overly optimistic about the consequences of participation.

This paper contributes to four literatures. First, it bridges a gap between disciplines by using standard economic methodology to inform a concern about the effects of incentives that is both widely-held in the applied ethics literature and highly influential in policy.[5] It is a part of the burgeoning literature on *repugnant transactions* (Kahneman, Knetsch and Thaler, 1986; Basu, 2003, 2007; Roth, 2007; Leider and Roth, 2010; Niederle and Roth, 2014; Ambuehl, Niederle and Roth, 2015; Elias, Lacetera and Macis, 2015a,b, Ambuehl and Ockenfels, 2017). While existing research characterizes people's motives for preventing others from engaging in voluntary transactions, the present paper focuses on a specific motive and examines whether its underpinnings stand empirical and theoretical scrutiny.

Second, it adds to the behavioral economics literature on incentives (see Kamenica (2012) for a review). It shows that incentives skew subjects' acquisition and interpretation of *external* information about a transaction in a way that amplifies their effect. This paper therefore complements a literature that studies the inferences that subjects draw from the incentive *per se* (Kamenica, 2008; Cryder et al., 2010; Bénabou and Tirole, 2006). In that body of work, reservation prices typically change in a way that diminishes the effect of incentives; for instance high incentives may signal that the transaction is unattractive, causing reservation prices to increase. This paper shows that people will behave as if trying to persuade themselves in cases where they mostly rely on *external* information about the transaction, whereas they will act in a manner consistent with the aforementioned literature if the main source of information about the deliverable is the incentive itself. The experiments control for other direct effects of incentives such as anchoring (Ariely, Loewenstein and Prelec (2003), see Maniadis, Tufano and List (2014) for a review), or motivation crowding (Titmuss (1970), see Frey and Jegen (2001) for a review).

[5]This paper cannot and does not take a stance on whether incentives for any particular transaction such as living organ donation should be limited. The welfare and policy implications are more nuanced; see Sections 4.4 and 6, respectively.

Third, it contributes to the literature on motivated reasoning by modeling the phenomenon in a fully rational setting with standard preferences (see Gino, Norton and Weber (2016); Bénabou (2015); Bénabou and Tirole (2016) for reviews in economics, and Kunda (1990); Klayman (1995); Epley and Gilovich (2016) for reviews in psychology). It therefore allows for an explicit test of the extent to which the phenomenon is consistent with Bayesian rationality.

Fourth, it contributes to the literature on endogenous information acquisition (Caplin and Leahy, 2001; Bénabou and Tirole, 2002, 2011; Suen, 2004; Koszegi, 2006; Eliaz and Spiegler, 2006; Gentzkow and Kamenica, 2011), specifically to the theory of rational inattention (see Caplin (2016) for a review). The experiment in Section 5 is an explicit experimental test thereof.

The remainder of this paper proceeds as follows. Section 2 briefly reviews some of the laws and guidelines that restrict incentives for transactions. Section 3 demonstrates the main behavioral patterns in an experiment that emphasizes ecological validity. Section 4 presents the theoretical model and relates it to welfare analysis. Section 5 conceptually replicates the first experiment, explicitly tests the model, and measures the extent to which empirical behavior conforms to Bayesian rationality. Section 6 discusses policy implications, and briefly outlines implications of this paper in fields such as finance, political economics, and health economics (in which medical experts charged with the acquisition and interpretation of information are influenced by incentives), as well as to information economics (informational moral hazard), and to marketing (bait and switch). Section 7 concludes.

2 Policies that restrict incentives

This paper identifies a mechanism with applications in diverse domains. Its focal motivation is the laws and guidelines that constrain incentive payments, a selection of which I review here. These regulations share three attributes: First, at the core, they all seek to protect the people targeted by incentives. Second, they neither intend to discourage the activities *per se* whose incentives they limit, nor do they merely attempt to prevent people from making potentially large mistakes. Rather, the concern is about *inducing* people to make decisions they might regret.[6] Altruistic participation, by contrast, is often applauded (e.g. Macklin, 1981). Third, each of these laws and guidelines covers a complex transaction and thus incorporates considerations that this paper does not address.

One category of such laws concerns research with human subjects, both non-medical (such as psychological science or experimental economics) and medical. Many are based on the Belmont

[6] This is particularly apparent when incentives are deemed *coercive* (Macklin, 1981; National Bioethics Advisory Commission, 2001; McGregor, 2005; Ripley, 2006).

Report (1978) which states that "undue influence ... occurs through an offer of an excessive ... reward or other overture in order to obtain compliance." The concern is that an offer may be "so excessively desirable that it compromises judgment" (Emanuel, 2004). Consequently, incentives that can be paid for research participation are limited in the United States (National Bioethics Advisory Commission, 2001), the European Union (Hughes et al., 2010), and many other jurisdictions.

Another category concerns transactions involving the donation of human tissue, such as kidneys (Open Letter To President Obama, 2014; Vatican Radio, 2014)[7] and bone marrow (World Marrow Donor Association (2011)). In both of these cases, a crucial argument is that incentives would distort prospective participants' assessment of the costs and benefits of the transaction, possibly to their detriment (Satz, 2010; Grant, 2011; Kanbur, 2004). The World Marrow Donor Association (2011), for instance, states that "remuneration may ... cause the prospective donor to withhold personal health information for fear of being disqualified from donation, preventing an accurate risk assessment and disclosure of risks specific to that donor."

Moreover, incentives are frequently restricted in the domain of reproductive technologies. While the U.S. permits commercial human egg donation, the American Society for Reproductive Medicine (2007) recommends that "payments to women providing oocytes should be fair and not so substantial that they ... lead donors to discount risks". Gestational surrogacy falls in the same category. The states Nevada, New Hampshire and Washington prohibit payments to surrogate mothers except for particular expenses that are explicitly listed in the states' statutes, out of concern for the surrogate mother.

Finally, many jurisdictions prohibit engagement in sexual activity for material compensation. A prominent view maintains that the decision to sell sex harms prostitutes, even if they choose to engage in the transaction of their own free will (Farley, 2013; Danna, 2014).

The claim that incentives skew the assessment of consequences associated with a participating in a transaction has also been made in various other domains. Incentives are outlawed, for instance, in student athlete recruiting on the grounds that they constitute "undue influence" (National Collegiate Athletic Association, 2015).

3 Incentives and information about a visceral transaction

In this section, I conduct a laboratory experiment to study how incentives affect the acquisition and interpretation of costly information about a transaction with which subjects are unfamiliar. The main test concerns a comparative static on the supply curve. If incentives truly skew the

[7] Paid living kidney donation is outlawed in every country of the world, except for the Islamic Republic of Iran (Rosenberg, 2015b).

acquisition and interpretation of information in the way the literature hypothesizes, and if that skewed information ultimately affects subjects' participation decision, then the elasticity of supply should be larger in cases where selective endogenous information acquisition is possible than in cases where it is not. Additionally, I study the direct effect of the incentive magnitude on information demand, as well as on the resulting changes in beliefs about what the deliverable entails and in reservation prices.

The experiment emphasizes ecological validity by using a real transaction about which subjects have homegrown beliefs and preferences. It is complementary to the experiment in Section 5, which trades off ecological validity for a greater ability to distinguish between mechanisms in a stylized setting with induced preferences (Smith, 1976).

The transaction in this experiment needs to have three key features. First, it should give prospective participants the opportunity to consult rich and multifaceted information and thus to access and interpret it selectively. Second, it should be unfamiliar and visceral. This property makes it difficult for subjects to assess the disutility of participation in monetary terms, which potentially renders reservation prices malleable. Third, it must be feasible in the laboratory. Hence, I have subjects decide whether to ingest whole insects in exchange for cash payment. This transaction is unfamiliar and aversive to the vast majority of participants, it is visceral, and there is rich and multifacted information available about insects as food.[8] Finally, the transaction is safe and feasible in the laboratory, since I use insects raised specifically for human consumption.

Clearly, insect eating differs on countless dimensions from transactions for which incentives are limited by law (examples include altruistic concerns and irreversibility). This experiment does *not* attempt to replicate these other dimensions, and does not need to. Indeed, it demonstrates that the attributes it does replicate are sufficient to cause skewed acquisition and interpretation of information, as well as corresponding changes in beliefs about what the deliverable entails and in reservation prices. The conclusions drawn from this experiment thus extend to any transaction that shares this limited set of attributes.[9]

3.1 Design

Structure. The experiment follows a 2×2 across-subjects design. The first dimension is the incentive amount, which takes a value of either \$3 or \$30 in exchange for eating an insect. The

[8] The transaction is intensely aversive to most subjects. Some reported that the experiment was "stressful" or that the "insects were scary", and others refused to even touch the containers they were packaged in. Even in countries such as China, Thailand, and Mexico, insect eating is not practiced by a wide majority. Rather, it is concentrated within particular regions and / or communities, and often limited to a small number of insect species. In my data, Asians and Hispanics are neither more nor less willing to eat insects than Caucasians.

[9] In other contexts, these effects may be counteracted by other forces.

second dimension is whether a subject can select and watch one of two videos about human consumption of insects (the *video* and *no video* conditions, respectively). A comparison between the latter two conditions permits the main test whether the supply of willing participants is more price-elastic when endogenous information acquisition is possible than when it is not.

Briefly, the experiment follows four stages (detailed below). First, receive an offer specifying the magnitude of the incentive. Although they are not yet asked for a decision, knowledge of the incentive may affect how they acquire and interpret information about the transaction. Second, subjects select and watch one of the two videos which emphasize, respectively, the upsides or downsides of eating insects (in the *video* treatment only). Third, they reveal their reservation price for eating insects. Fourth, they decide whether to eat the insect for the incentive amount they were initially promised. I incentivize subjects to pay attention to the videos and to use them as the main source of information about the transaction by asking them to make all decisions without having seen the actual animals they may be about to consume. Subjects then participate in ancillary treatments. The rules for payment and consumption of insects ensure that it is in the subjects' best interest to reveal their genuine preferences in each decision.

Specifics of the main stages (1 - 4). In stage 1, subjects learn the incentive amount they will be offered, and that they will decide, for each of five food items, whether to eat the item in exchange for that amount. Only at this point do they learn that all of the food items are whole insects that are either baked, or cooked and dehydrated, and produced for human consumption. Subjects know of both incentive amounts and that they are randomly assigned to one of them.[10] Hence, they cannot *rationally* draw inferences about the experience of eating insects. While effects such as anchoring may still apply, the *no video* condition serves as a control that allows me to difference out any effects pertaining to the payment amounts *per se*.[11]

Only subjects in the *video* condition participate in stage 2; those in the *no video* condition proceed directly to stage 3.[12] Participants choose between watching a 6-minute video entitled "Why you may want to eat insects" (the *encouraging* video) and one called "Why you may not

[10]In contrast, subjects in the video condition are unaware that other subjects cannot acquire any information, and vice versa.

[11]Payment amounts *per se* may affect subjects' decisions through at least four channels. First, because subjects are informed of both payment amounts, they might be unhappy if they receive the $3 incentive. Second, eating insects for less compensation may give the subject more bragging rights. Third, a high incentive amount may crowd out intrinsic motivation to eat insects (Frey and Jegen, 2001). Fourth, incentive amounts may serve as an anchor for subsequent decisions (Maniadis et al., 2014).

[12]Due to this setup, subjects in the *video* condition have more time to contemplate their choice, and thus additional opportunity to think about the transaction, perhaps in a way that depends on the incentive amount. The objective is *not* to identify the effect of the specific menu of videos I offer, but rather to identify the effect of opportunities for selective acquisition and interpretation of information in general. The additional contemplation time in the *video* treatment furthers that objective.

want to eat insects" (the *discouraging* video). These video titles, and the approximate 6-minute duration are all the information subjects have when making that choice.[13] To capture the idea that information is costly, subjects are not allowed to watch both videos. Moreover, to ensure sufficient statistical power, each subject in the *video* condition must watch at least one video. In this sense, subjects can choose the type, but not the amount of information to acquire. (The experiment in Section 5 complements this design choice in that it places no restrictions on the kind or amount of information a subject can acquire.) Subjects watch the selected video directly after choosing it. Because the videos are fairly long and contain significant detail, incentives may affect not only which video a subject chooses, but also which parts of the video the they pay attention to.[14]

In stage 3, subjects reveal reservation prices by filling in a multiple-decision list for each of the following five food items: 2 house crickets, 5 large mealworms, 3 silkworm pupae, 2 mole crickets, 2 field crickets. On each line in each list they decide between the options "Get $\$p$. In exchange, eat the food item" and "Do not participate in this transaction," for multiple values of p ranging from \$0 to \$60 in 21 increasingly large steps.[15] They click on the line at which they prefer to switch from refusing the transaction to accepting it; the remaining choices are filled in automatically.

In stage 4, subjects decide, separately for each of the five food items, whether to eat the insect in exchange for the incentive they were promised in stage 1.

Payment and execution of consumption decisions. Each subject makes many decisions. To incentivize truthful revelation of preferences, exactly one of all decisions is randomly chosen for implementation at the end of the experiment. The selected decision entirely determines a subject's payment and consumption of insects. The implementation probability varies across decisions. There is an 80% chance the decision selected for implementation is from stage 4. The reason is that those decisions are not only an outcome measure, they are also intended to influence how subjects acquire and interpret information about insect consumption. The strength of the latter effect likelyincreases with the chance that a stage 4 decision will be implemented. Table 1 details the implementation probability of the remaining stages.

[13] Each video lists various reasons for or against human insect consumption. Transcriptions and links to the videos are included in Appendix D.

[14] Subjects in the *video* condition also select at least four out of a selection of 9 video clips, grouped in bins of three named "Reasons for eating insects", "Reasons against eating insects", "Other information about eating insects". Subjects face a 3% chance of watching the selected clips, and a 97% chance of watching the selected 6-minute video. See Appendix A.6 for details and analysis.

[15] The amounts are 0, 1, 2, 3, 4, 6, 8, 10, 12.5, 15, 17.5, 20, 22.5, 25, 27.5, 30, 33, 36, 39, 44, 50, 60. Resolution is finer at lower levels to increase statistical power in light of the positively skewed distribution of reservation prices. The amount \$3 was not included in the decision lists for the first 79 subjects.

		Implementation probability
	Main stages	
1.	Learn incentive amount to be offered in stage 4: \$3 or \$30.	
2.	Select encouraging or *Discouraging* video and watch it (*video* condition only).	
3.	Reveal reservation price for each food item.	7%
4.	Participation decision for each food item.	80%
	Ancillary stages	
5.	Insects are handed out.	
6.	Reveal reservation price for each food item.	7%
7.	Predict others' reservation prices.	6%
	Implementation	
8.	One decision randomly selected for implementation, insects consumed (if selected)	

Table 1: Experiment timeline. Instructions for stages 1 through 6 are read out aloud in the beginning of the experiment. Stage 7 is a surprise. Instructions are displayed on subjects' screens immediately before that stage.

All subjects are assured that all insects will be consumed in the privacy of a visually secluded space in the presence of only the experimenter who ensures that the subject consume the animals in entirety. This minimizes social motives, such as trying to impress others.

Subjects make many consumption decisions before seeing the actual insects, and thus may be unpleasantly surprised (see Appendix D for pictures of the food items). While I cannot force participants who change their mind to ingest insects against their will, the design must nonetheless ensure that participants do not accept transactions with the expectation of a reneging option on the decision selected for implementation. Hence, a subject who reneges on the selected decision not only forfeits whatever she would have received for eating the insect, but must also pay an additional fee of \$20 (subjects who rejected the selected transaction cannot renege). This penalty is taken out of a \$35 completion payment that the subject would otherwise receive.[16] Subjects are aware of all of these rules from the outset of the experiment.

Ancillary stages (5 - 7). In stage 5, all subjects receive five containers. Each is filled with insects and a folded piece of paper with a code. They must enter all codes into the computer, which forces them to open each container, remove the label from within and thus view and

[16] The Stanford Institutional Review Board would not give ethics approval for this experiment until I suggested to present this penalty to subjects as follows: "You will be paid at least \$15 if you complete this experiment, regardless of your choices. In addition ... you automatically receive \$20 if you ... follow through with the decisions you make"

(inadvertently) smell each of the insects.[17] In stage 6, they again reveal their reservation prices.

In order to measure whether subjects are aware of the effects of incentives on others, stage 7 asks them to predict the reservation prices of other participants. Subjects make separate predictions for other participants in the \$3 and in the \$30 incentive conditions, but only for participants in the same video condition as themself.[18] Because this stage comes as a surprise, subjects' own decisions are not influenced by considerations of how others would decide.[19] There is a 6% probability that the accuracy of a randomly-selected prediction will determine a subject's payment. If so, she will not consume any insects, and her \$35 completion payment is reduced by \$0.50 for each \$1 that her prediction differs from the true mean.[20]

3.2 Implementation and preliminary analysis

Implementation A total of 671 subjects participated in one of 39 computerized sessions in May, June, and July 2015 at the Ohio State University (499 subjects), Stanford University (110 subjects), and the University of Michigan (62 subjects). 271 subjects participated in the *no video* treatment (136 and 135 with \$3 and \$30 incentives, respectively), and 400 participated in the *video* treatment (197 and 203 with \$3 and \$30 incentives, respectively).[21] A large number of subjects is required since individuals' willingness to eat insects is highly heterogenous. Each session lasted about 2.5 hours and contained both payment conditions. Either all or none of the subjects in a session were in the *video* condition. At the beginning of the each session I read the instructions aloud.[22] I recruited subjects using the universities' experimental economics participant databases. The invitation emails mentioned that the experiment would involve the

[17]As a filler task during the handing out of the insects, subjects complete an extended version of the Cognitive Response Scale (Toplak, West and Stanovich, 2014), and sets D and E of Raven's (1960) standard progressive matrices.

[18]Subjects first make a prediction for an average participant. They then separately predict the mean reservation price of those who were offered \$3 and \$30, respectively, in randomized order.

[19]Subjects were informed at the beginning of the experiment that stage 6 would be followed by additional decisions, that they would learn later what they entail, and that the chance that one of them would determine their payment was 6%.

[20]A subject thus maximizes her expected payoff by stating the median of her beliefs about the true mean.

[21]See Appendix Table A.5 for summary statistics about the subjects.

[22]Instructions are reproduced in Appendix D. The experiment was coded in Qualtrics. I had expected a much larger number of subjects at the University of Michigan, but only 62 were available. The fraction of subjects in the *video* condition exceeds 50% because only that condition reveals information choice. The 79 Stanford students who first participated in this experiment were not given any decisions regarding field crickets. The 48 first Stanford students also did not make any predictions about other participants. In addition, 68 Stanford students participated in an exploratory treatment. Data from the exploratory treatment are not included in any analysis as that treatment was poorly calibrated. The vast majority of participants saw highly visceral images of insects, which muted the effects of endogenous information acquisition.

consumption of food items on the spot, but did not mention insects.[23] It asked recipients not to participate if they have food allergies, are vegetarian or vegan, or eat kosher or halal.

Subjects could renege on the decision selected for implementation in exchange for \$20 if they had agreed to eat an insect. Five participants (0.8%) chose to do so. All of them were in the \$30-condition.[24]

Randomization check. Randomization into treatments was successful. Of 24 F-tests for differences in subjects' predetermined characteristics across the four treatments, only one is significant at the 5% level, and four more are significant at the 10% level. This falls within the expected range. Details are in Appendix A.1.

Summary statistics. Eating insects is aversive to most participants. For each of the five species, column 1 of Table 2 lists the fraction of subjects who have a positive reservation price in stage 3. For each item, at most 5% of subjects would eat it for free. The median reservation price is substantial, ranging from \$9 to \$18.75 (column 2). There is also a substantial percentage of subjects who would not eat the insect even for the highest incentive amount offered in the multiple price lists (\$60), ranging from 18% to 30% (column 3). Nine percent are not willing to eat any insect for any price in any multiple-price list.[25]

	(1)	(2)	(3)
	Reservation price		
	Fraction $>$ \$0	Median	Fraction $\geq$ \$60
2 house crickets	0.96	9.00	0.18
5 large mealworms	0.96	18.75	0.30
3 silkworm pupae	0.95	13.75	0.23
2 mole crickets	0.96	13.75	0.24
2 field crickets	0.95	13.75	0.22

Table 2: Summary statistics of reservation prices elicited in stage 3. \$60 is the highest price offered in the multiple-price lists. 9% of subjects reveal a reservation price $\geq$ \$60 for all five species. Data pooled over treatment conditions. Interval midpoints are used for analysis. $n = 671$.

[23] An exception exists in the invitation emails in Michigan, and those for the last 31 Stanford subjects, which mentioned that the experiment involves the voluntary consumption of food items, including edible insects. This information had no statistically measurable effect on the fraction of participants who refused to eat an insect for any price offered.

[24] Four of the five were in the video condition, and three had opted for the encouraging video.

[25] Each decision made in stage 4 of the experiment is also made as a part of a multiple-price list in stage 3. These decisions are sometimes inconsistent. See Appendix A.2 for details.

Analysis. In all analyses, I control for subjects' gender, ethnicity, as well as a second order polynomial in age,[26] and I include university and species fixed effects. All constants are estimates for the mean participant. Data from the multiple-price lists are interval-coded. I use interval midpoints for analysis. The results are robust to alternative specifications (see Appendix A.7).

3.3 Main analysis

Result 1: Higher incentives raise the demand for encouraging information. With higher incentives, subjects are more likely to demand information that encourages rather than discourages eating insects. The fraction of subjects choosing the discouraging video drops by over a third, from 18.3% to 11.3%, as incentives rise from \$3 to \$30. The effect size of 7.01 percentage points is measured with a standard error of 3.60 percentage points (clustered by subject, $n = 400$, linear regression).[27]

Hence, incentives skew subjects' information acquisition. This then raises the question: are subjects merely selecting the video that makes it easiest to follow through with the choice they would have made anyway, or does the skewed information acquisition alter the subject's ultimate participation decision?

Result 2: The supply of participants is more elastic when endogenous information acquisition is possible. I now study the supply curves. Does the selective information acquisition induced by the incentive ultimately affect subjects' consumption decision, or does it merely rationalize a decision subjects would have made anyway? I observe the decision subjects would have made anyway in the *no video* condition, since it does not allow for endogenous information acquisition. If selective information acquisition induced by incentives indeed affects subjects' participation decisions, the slope of the supply curve in the *video* condition should differ from that in the *no video* condition.

Panel A of Table 3 shows that 37.17% of subjects in the *no video* condition are willing to eat insects in exchange for \$3 (averaged across the five species). This number rises to a significantly higher 59.57% if the incentives are raised to \$30, an increase of 22.39 percentage points. In this condition, incentives work because a higher incentive exceeds the reservation price of a larger number of subjects.

In the *video* condition, the supply response also depends on another mechanism. Due to incentives' effect on how subjects acquire and interpret information, a change in the incentive

[26]Due to the presence of some outliers in age.
[27]See Appendix A.7 for Probit and Logit specifications.

amount may now also *change* reservation prices. If skewed information acquisition leads subjects to convince themselves to participate when incentives are high, participation rates will now respond even more strongly to an increase in the incentive.

Indeed, in the *video* condition, increasing the incentive from \$3 to \$30 raises the participation rate from 37.69% to 70.53%. This is an increase of 32.84 percentage points, which is 10.44 percentage points more than the effect produced by the same \$3 to \$30 increase for the *no video* condition. The supply response in the *video* condition is nearly one-and-a-half times that in the *no video* condition, as Panel A of Figure 1 illustrates. Incentives skew information acquisition, which in turn affects subjects' participation decisions.

This analysis intentionally does *not* condition on the video each subject chooses to watch, for two reasons. First, the choice of information is one possible mechanism driving the behavior that this experiment is designed to document. Statistically muting this mechanism would defeat the purpose of the experiment. Second, the choice of video is not the only mechanism that could drive the effects. Incentives may also change how subjects pay attention to, and let themselves be persuaded by the chosen video. This is plausible since the videos contain a variety of arguments, each of which one may deem more or less convincing, depending on incentives.[28]

Result 3: Higher incentives lower reservation prices. I now analyze the effect of incentives on reservation prices. They capture subjects' expectations about the disutility of eating insects.

This analysis presents two challenges, one experimental, the other statistical. Experimentally, the fact that subjects are first presented with an incentive amount, and are then asked to reveal their reservation price potentially invites anchoring effects (Maniadis et al., 2014). To illustrate, the offer of \$15 that subjects encounter in the multiple-decision lists may look quite attractive to someone who was initially promised an incentive of \$3, but would appear diminutive to someone who had been promised \$30. Through anchoring, incentives raise reservation prices.

If, however, larger amounts of money cause subjects to convince themselves that insect-eating is not as bad as they might otherwise have thought, then a higher incentive would lower reservation prices. For instance, a subject may not even begin to consider eating insects for

[28] Conditioning on the choice of video would seem to allow for the quantification of the relative importance of the two effects. Such an analysis, however, would be confounded by the endogenous selection of the video. Instead, I show that changes in how subjects interpret information must play a role, in two ways. First, Section 3.4 shows that the choice of the video alone suffices to explain neither the difference in the slope of the supply curves, nor the effect on reservation prices. Hence, incentives must also cause subjects to interpret the same video differently. Second, the experiment in Section 5 does not offer an explicit choice between sources of information, and hence must work entirely through differential interpretation of the same information.

	A. Supply curves			B. Reservation prices		
Incentive	\$3	\$30	*Difference*	\$3	\$30	*Difference*
Information						
no video	37.17	59.57	22.39***	20.86	26.83	5.97***
	(3.39)	(3.41)	(4.78)	(1.76)	(1.59)	(2.26)
video	37.69	70.53	32.84***	20.79	19.51	-1.28
	(3.01)	(2.55)	(3.93)	(1.60)	(1.45)	(2.10)
Difference	0.52	10.96**	10.44*	-0.07	-7.32***	-7.25**
	(4.56)	(4.33)	(6.19)	(2.32)	(1.99)	(3.06)

Table 3: Panel A shows the percentage of participants who are willing to eat the food item for the offered incentive amount (averaged over the five food items), by treatment. Panel B shows estimates of mean reservation prices in dollars elicited in stage 3 by treatment, amongst those who are willing to eat at least one species of insect in exchange for \$60 or less. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Asterisks are suppressed for levels.

incentives of a mere \$3, and \$15 may not make her change her mind, either. But an incentive of \$30 at the beginning of the experiment may induce her to start contemplating what was previously unthinkable, and even convince herself that eating insects is not all that bad. With this different viewpoint, she may now even accept the \$15 offer in the multiple-price list. After all, she has already convinced herself.

The anchoring mechanism relies merely on the presence of different promised incentives that precede the elicitation of reservation prices. Anchoring should thus be present to comparable extents in both the *video* and *no video* conditions. Selective information acquisition, by contrast, is limited to the *video* condition. Therefore, I will measure the effect of incentives on expectations by the *difference* in their effect on reservation prices across the *video* and *no video* treatments.

The econometric challenge here consists of subject heterogeneity and censored observations. While the vast majority of subjects are willing to eat insects once the incentive becomes sufficiently large, a minority are opposed to the idea of eating any insect for any compensation in the experiment, including the maximum of \$60. For those subjects, neither \$3 nor \$30 is a high incentive. Hence, one cannot reasonably expect such subjects to respond to the incentive variation. Additionally, even if a subject's reservation price is less than \$60 for some species, it may exceed that threshold for others, and will thus be censored.

I estimate the effect of incentives on the reservation prices of subjects who are willing to eat at least one species of insect in exchange for \$60 or less. I use a version of Cragg's (1971) double hurdle model, which endogenously determines the incidence of the two types of subjects and simultaneously accounts for censoring. I account for the panel structure of the experimental data using the methods in Dong, Chung and Kaiser, 2004 and Dong and Kaiser, 2008.[29]

The results are shown in Panel B of Table 3 and Figure 1. In the *no video* treatment, there is a sizable anchoring effect. The increase in the incentive from \$3 to \$30 leads to a \$5.97 increase in reservation prices, from \$20.86 to \$26.83. If incentives do not lead subjects to form different expectations about the experience of insect-eating, then we should expect to see a similar change in reservation prices for the *video* condition. The data, however, show clear evidence to the contrary.

For subjects in the *video* treatment, the increase in the incentive leads to a *decrease* in the average reservation price, from \$20.79 to \$19.51. The difference in the effects across the *video* and *no video* conditions is a significant \$7.25. Hence, incentives change subjects' expectations by influencing how they acquire and interpret information.

These effects persist beyond the distribution of the insects. Receiving the insects lowers the mean reservation price in the (*video*, \$30 incentive)-treatment by \$2.52, raises it in the (*no video*, \$3 incentive)-treatment by \$1.24, and leaves it close to unchanged in the remaining conditions. The difference-in-differences across treatments remains virtually unchanged.[30]

Result 4: Subjects are not fully aware of these effects, neither in others, nor in themselves. To study subjects' awareness of the effects of incentives, I use stage 7 of the experiment, in which subjects are incentivized to guess other subjects' reservation prices to the best of their abilities. Subjects make separate guesses for those in the \$3 and \$30 conditions, respectively. Subjects thus reveal how they expect incentives to affect others' reservation prices. I compare the actual effect of incentives to these beliefs (see Appendix A.4 for details). To disentangle anchoring from the effects of skewed information acquisition, recall that subjects guess the reservation prices of others who were in the same *video* / *no video* condition as themselves. Data from subjects in the *no video* condition reveal whether they anticipate anchoring; data from those in the *video* condition show whether they correctly predict the combined effect of selective information acquisition and anchoring.

Recall that higher incentives raise reservation prices in the *no video* condition. In stage 7, the average subject's prediction is very accurate. Not only do subjects correctly predict the sign

[29] I use the Stata implementation by Engel and Moffatt, 2014. See Appendix A.3 for details. The Cragg (1971) double hurdle model nests the Tobit model. Both models consist of two terms, one that determines the probability that an observation is censored, and one that determines the distribution of uncensored observations. The Tobit model imposes the restriction that any explanatory variable enters each of these terms with the same coefficient, the Craggit model does not. I test and reject this restriction in Appendix A.3.

[30] See Appendix A.5 for details.

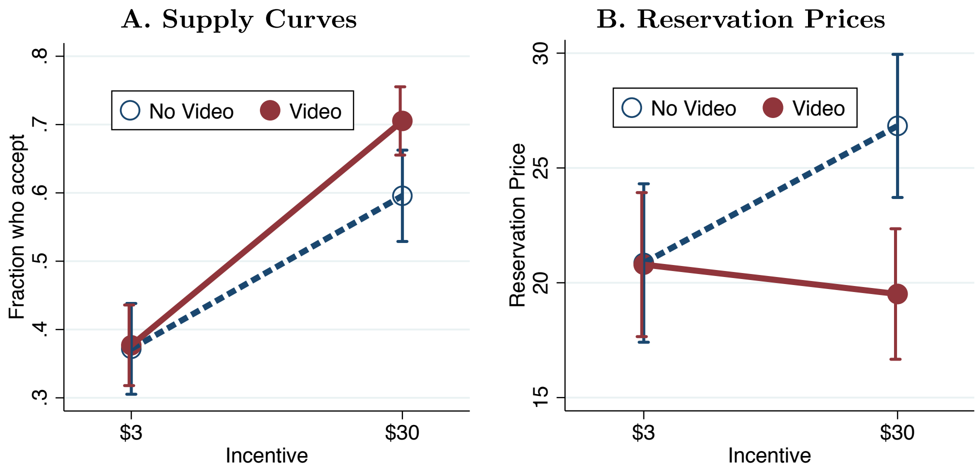


Figure 1: Panel A shows how the treatments affect the fraction of subjects willing to eat insects for the promised incentive. Panel B shows how the treatments affect reservation prices. Data in both panels are taken from Table 3.

of this effect, they also accurately guess its magnitude ($p > 0.5$ for a test of the Null hypothesis that predicted and actual effect sizes are equal).

The *video* condition offers a striking contrast. Subjects in that condition make predictions about others who are also in the *video* condition. These predictions are directionally wrong and far off in magnitude ($p < 0.01$) . In fact, predicted incentive effects are the same regardless of whether they concern others in the *video* condition or others in the *no video* condition. Hence, subjects correctly predict that others are subject to the anchoring effect, but they entirely fail to anticipate that incentives also cause others to selectively acquire and interpret information in a way that alters reservation prices.

Not only do subjects fail to predict the effects of skewed information acquisition in others, they also misperceive the extent to which they are affected themselves. To see this, recall that subjects in the *video* condition who face the \$30 incentive have a lower average reservation price than those who are given the \$3 incentive. Subjects in the former condition also make predictions which are, on average, \$2.62 lower than those than those in the latter condition ($p < 0.05$). Hence, subjects in the (*video*, \$30 incentive)-condition appear to first convince themselves that insect eating is not that bad, and are then unable to fully unbias their beliefs when asked to predict the reservation prices of others, in spite of monetary incentives for accuracy.

Mechanism: Choice between videos or different interpretation of a given video? Incentives may affect participation decisions and reservation prices not only through the choice between the videos, but also by changing which parts of a given video a subject pays attention to, and how convincing she deems it. Two back-of-the-envelope calculations reveal that the data are at least partially driven by the latter mechanism.[31] First, higher incentives lead to an additional increase in participation of 10.44 percentage points in the *video* treatment compared to the *no video* treatment. This exceeds the 7.01 percentage point increase in the number of subjects choosing the encouraging video, which therefore falls short of explaining the effect on participation. Second, suppose that the entire difference-in-difference estimate of the effect on reservation prices of \$7.25 is wholly due to subjects' choice of video (rather than a differential interpretation of it). Since subjects in the \$30-condition chose the encouraging video only 7.01 percentage points more often, it follows that in order to explain the effect of \$7.25 on reservation prices, the effect of watching the encouraging rather than the discouraging video on reservation prices would need to be on the order of \$7.25 / 0.07 = \$103.55. This is much more than the largest difference that could possibly be measured by the price lists (\$60). In fact, the reservation prices of those who watched the discouraging video exceed those of other subjects in the *video* treatment by only \$10.60 (standard error 3.59).[32]

3.4 Alternative interpretations

Through the four results above, the experiment paints a coherent picture. Incentives affect how subjects acquire and interpret information in a way that systematically skews their beliefs about what the transaction entails, and this change in beliefs affects the participation decision that subjects ultimately make. For each individual result, there are alternative explanations that I address here. None of the alternatives, however, can naturally explain the entire collection of findings. Because this experiment emphasizes ecological validity and thus involves homegrown preferences and beliefs, however, the rigor with which I can separate mechanisms is limited. The complementary experiment in Section 5 addresses this point by trading off ecological validity for the ability to precisely delineate mechanisms.

***Ex-post* rationalization and cognitive dissonance.** It is conceivable that a subject decides to accept the transaction immediately upon being notified that her incentive is \$30 (or to immediately refuse for an incentive of \$3), and merely chooses between videos as a means

[31] Additionally, Appendix A.8 analyzes the data of only those subjects who opted for the encouraging video, and finds qualitatively unchanged results. That analysis ignores the endogeneity of the video choice.

[32] Clustered by subjects, estimated using university and species fixed effects. This statistic reflects a choice. It is therefore not an unbiased estimate of the effect of watching one over another video.

to rationalize this choice *ex post*, for instance, to resolve cognitive dissonance. Such *ex-post* rationalization can explain the results for both video choice and reservation prices. It is inconsistent, however, with the fact that supply is more elastic in the *video* condition than in the *no video* condition. According to *ex post* rationalization, the information in the video should not change whether subjects decide to eat insects for money since that decision has already been made. The induced change in supply elasticities, however, shows that skewed information acquisition *does* in fact affect participation decisions.[33]

Can anchoring alone explain the findings? In this experiment, incentives affect reservation prices differently when endogenous information acquisition is possible than when it is not. A possible explanation could be that the information in the *video* condition simply eliminates anchoring without affecting beliefs about the experience of eating insects. *A priori*, this explanation conflicts with a literature that documents substantial anchoring effects even when subjects have nearly complete information about the transaction they are evaluating (Ariely et al., 2003).[34] *A posteriori*, the data refute the hypothesis in two separate ways. First, subjects in the video treatment who receive the high incentive predict significantly *lower* reservation prices for others. They must have convinced themselves that eating insects is not as aversive as previously thought, and are unable to unbias these beliefs in spite of incentives for accuracy. If beliefs and reservation prices were influenced by anchoring alone, we would expect no such result, or the opposite. Second, this hypothesis does not easily explain why the the fraction of subjects willing to eat insects for $30 is larger in the *video* than in the *no video* condition. For anchoring to explain this effect, it is not sufficient that valuations are drawn towards the anchor, they would—implausibly—need to overshoot: in the *no video* condition, the $30 anchor would have to increase reservation prices from a value below $30 to some value above $30.

Additionally, the experiment in Section 5 addresses concerns about anchoring where that mechanism cannot play a role by design.

Confirmation bias and positive testing bias. The psychology literature on confirmation bias and positive testing bias shows that people often seek and interpret information in a way that tends to confirm the hypotheses they currently hold (Klayman, 1995; Rabin and Schrag, 1999). The results of this experiment can be interpreted as another manifestation

[33] The data are consistent with the following, more involved account of *ex-post* rationalization, however. Subjects might anticipate how easily they will be able to rationalize their choice, and might be more likely to accept the transaction if it is easier. This mechanism relates to the idea that subjects may select particular sources of information, not because they merely affect beliefs about the experience of eating insects, but because they directly affect that experience. The experiment in Section 5 precludes this explanation.

[34] Participants in Ariely et al. (2003) were given a sample of the aversive stimulus (obnoxious noise) *before* they were subjected to the anchor and revealed their reservation price to listen to more of the same noise.

of this tendency. Additionally, however, they show the hypothesis that is being tested. If incentives are high (low), individuals start with the mindset that participation (abstention) is the optimal choice. This is not trivial; one could alternatively imagine high incentives triggering heuristics that make people more keen on exploring the potential downsides of the incentivized transactions, which would result in the opposite effect.

Beliefs or preferences? Information acquisition has two potential effects that the present experiment cannot disentangle. On the one hand, information can change beliefs about the costs and benefits of eating insects without affecting the actual sensory experience. On the other hand, information might make it easier or harder to eat a given insect. The experiment in Section 5 mutes the latter channel by design and demonstrates that incentives lead to similar effects as in the current experiment when belief changes are the only possible mechanism.

4 Information acquisition about a transaction by a Bayesian agent

In this section, I present a model of costly information acquisition to study how incentives affect a rational Bayesian agent's information acquisition and beliefs about the deliverable of a transaction. I show that even though the findings of the first experiment in this paper confirm the behavioral hypotheses of the ethics literature on incentives, such behavior is not necessarily a cause for concern. Specifically, higher incentives typically induce rational Bayesian agents to demand and interpret information in a way that is more favorable to participation, thereby altering her expectations about what the deliverable entails. The model helps clarify the welfare standards under which this behavior may justifiably be regarded as concerning. It also highlights how to empirically determine whether these conditions are satisfied. All proofs are in Appendix Section B.3.

4.1 Setting

An agent decides whether or not to participate in a transaction in exchange for a material incentive m. He is uncertain about what the deliverable entails. The (dis)utility of providing the deliverable depends on an unknown state $s \in \{G, B\}$. The state is good ($s = G$) with prior probability μ, and bad ($s = B$) otherwise. If the agent participates and the state is good, the total utility derived from providing the deliverable and obtaining the incentive amount is positive, $\pi_G + m > 0$. If he participates and the state is bad, providing the deliverable yields disutility π_B such that total utility is negative $\pi_B + m < 0$. If the agent does not participate, he receives utility of 0. Hence, he would like to participate in the good state, and abstain in the bad state.

Before the agent makes her participation decision, he can acquire information about the state, and thus arrive at a posterior belief that the state is good. If the posterior is sufficiently high, the agent participates; otherwise he abstains. If the posteriors are non-degenerate, the agent thus faces a positive probability p_B of participating in the bad state (committing a false positive error) and a positive probability $1 - p_G$ of abstaining in the good state (committing a false negative error). Different information structures lead to different distributions of posterior beliefs, and therefore to different bundles of state-contingent participation probabilities (p_G, p_B). Therefore, the choice of information is equivalent to a choice between pairs of false positive and false negative probabilities. In this model, the agent chooses these probabilities directly.

Crucially, information acquisition is costly. The cost of the information associated with a pair of state-contingent participation probabilities (p_G, p_B) is given by the real-valued differentiable function $\lambda \cdot c(p_G, 1 - p_B)$, where $\lambda > 0$ is a constant that parametrizes the marginal cost of information. This assumption best fits an environment that is informationally rich, so that many information structures and thus many pairs of state-contingent participation probabilities are available. I assume that c satisfies four conditions: First, c is increasing. This implies that better information is more costly. Second, c is convex. This captures the idea that marginal decreases in the false positive and false negative probabilities are more costly the more information one already has. Third, since the agent can implement any choice probability that does *not* depend on the state of the world without acquiring any information, I assume that $c(q, 1 - q) = 0$ for all $q \in [0, 1]$. Fourth, I ensure an interior solution by assuming that $\lim_{p_G \to 1} c(p_G, 1 - p_B) = \infty$ for all $p_B < 1$, and $\lim_{p_B \to 0} c(p_G, 1 - p_B) = \infty$ for all $p_G > 0$. These conditions encompass Shannon mutual information costs, the workhorse assumption of the rational inattention literature (Sims, 2003, 2006; Matějka and McKay, 2015).

Choosing state-contingent participation probabilities in practice. Sequential information acquisition (Wald, 1947) is an example of how one can plausibly choose state-contingent participation probabilities in practice. To see how, consider an agent who acquires information in small pieces, such that his posterior belief about the state evolves over time. The agent obeys the following decision rule. He decides to participate as soon as his posterior about the state being good is sufficiently high, and he decides to abstain as soon as the posterior is sufficiently low. Otherwise, he continues acquiring information. The choice of the threshold values that trigger a decision corresponds to a choice of p_G and p_B. For instance, an agent who chooses an upper threshold that is far from his prior implements a low false-positive probability. Figure 2 illustrates a decision rule that leads to low p_G and low p_B (participation / abstention thresholds 1), and another rule that leads to higher p_G and p_B (participation / abstention thresholds 2).[35]

Interpretation of state-contingent payoffs. In this model, a decision maker commits a false positive or false negative error if he takes an action that differs from the one he would have taken under full information. Hence, one may usefully interpret π_G (π_B) as the change in expected lifetime utility of a prospective participant for whom participation (abstention) is the optimal action, conditional on all information about the utility consequences for that decision maker that is potentially available at the time of the decision. Due to individual heterogeneity, different people may face different states.[36]

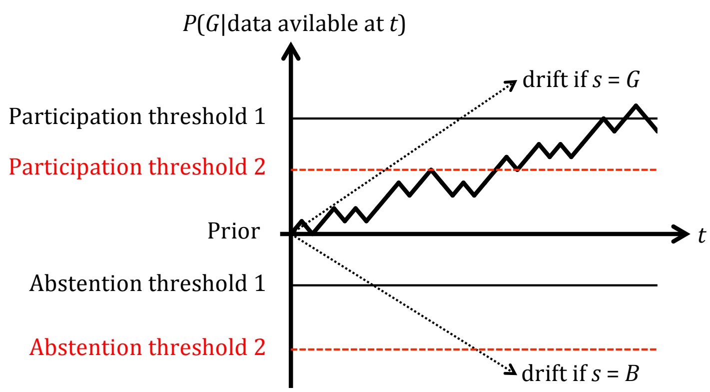


Figure 2: Selecting (p_G, p_B) with sequential information acquisition. If $s = G$, then $P(G|\text{data})$ drifts upwards, otherwise it drifts downwards. The decision maker keeps acquiring information until $P(G|\text{data})$ hits an upper threshold, in which case he participates, or until $P(G|\text{data})$ hits a lower threshold, in which case he abstains. The further a threshold is from the prior, the less likely the subject makes the corresponding decision in error. In this example, both p_G and p_B are smaller for the solid, black thresholds (thresholds 1) than for the red, dashed ones (thresholds 2).

[35] Morris and Strack (2017) and Hébert and Woodford (2017) formally relate information cost functions across sequential and static information acquisition problems.

[36] In the case of kidney donation, for instance, these values incorporate the change in expected quality-adjusted years of life. Accordingly, information acquisition extends beyond medical facts. The agent is interested in comprehensive information about how donation affects his life, for instance through constraining the professional and leisure activities he may want to pursue. It is also up to him to determine the extent to which medical consequences will affect his well-being (such as fatigue that may arise as a side effect of donation (Tellioglu et al., 2008; Beavers et al., 2001)). The ethics literature on informed consent argues explicitly for such a comprehensive definition of understanding the intervention (Faden and Beauchamp, 1986).

4.2 Analysis

In this section, I characterize the comparative statics of optimal information acquisition regarding incentive amount m. I begin by characterizing the agent's objective function. If the agent selects state-dependent choice probabilities (p_G, p_B), he obtains the upside payoff $\pi_G + m > 0$ with probability $\mu \cdot p_G$, and the downside payoff $\pi_B + m < 0$ with probability $(1-\mu) \cdot p_B$. With the remaining probability, he does not participate in the transaction and obtains utility 0. Hence, his *ex ante* expected utility, excluding costs of information, is $U(p_G, p_B; m) = \mu p_G(\pi_G + m) + (1-\mu)p_B(\pi_B + m)$. The decision maker chooses the pair of probabilities (p_G, p_B) to solve the following problem.

$$\max_{p_G, p_B} U(p_G, p_B; m) - \lambda c(p_G, 1 - p_B) \tag{1}$$

How does the solution to this problem depend on the monetary incentive m? The answer is most easily seen graphically. Figure 3 depicts a part of the agent's choice set.[37] The vertical axis depicts p_G, the horizontal axis depicts $(1 - p_B)$. Both of these are goods; they are the probabilities of avoiding an erroneous decision, conditional on the state. Hence, the higher up an agent's chosen bundle, the smaller is the probability of a false negative. The further right the chosen bundle, the smaller the probability of a false positive. I separately plot the level curves of U and those of the cost of information function c on this space. The level curves of U are straight and parallel lines, since U is a linear combination of p_G, $(1 - p_B)$ and a constant. U increases towards the upper right corner of the graph. The level curves of c are concave, since c is convex.

The agent chooses whether to acquire any information at all, and if so, what information, and hence which bundle (p_G, p_B), to select. Throughout, I consider the case in which he does acquire information. Hence, for an initial level of material compensation m, the agent's optimal choice may be a vector such as point A in the figure.

The total effect of an increase in m derives from a substitution effect and a stakes effect. We obtain the former by temporarily interpreting problem (1) as the Lagrangian for the maximization of U subject to a constraint on information acquisition costs, $c(p_G, 1 - p_B) = \bar{c}$ for some fixed $\bar{c}$. An increase in m raises the weight of the good p_G in the utility function U and lowers that of $(1 - p_B)$. Intuitively, the increase in the weight on p_G reflects the increased opportunity cost of non-participation, whereas the decrease in the weight on $(1 - p_B)$ reflects the fact that higher incentives partially insure against adverse outcomes. Hence, the indifference curves tilt to the left, and the constrained optimum shifts upwards and to the left; for instance to a bundle

[37] The agent's choice set is $[0, 1] \times [0, 1]$, and bundles where $p_B > p_G$ are dominated. For ease of exposition only a subset is depicted.

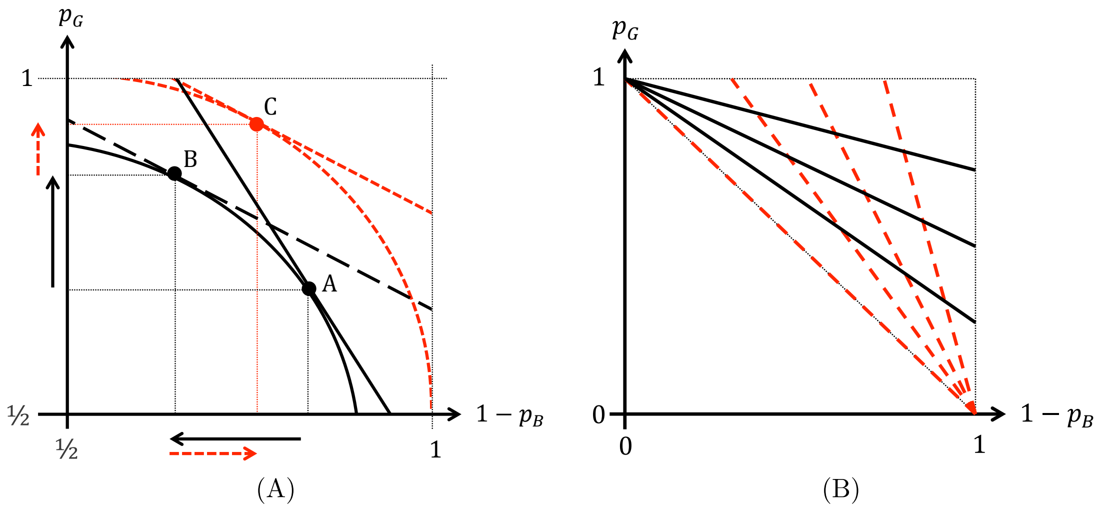


Figure 3: Panel A. Effects of an increase in the incentive amount m. The horizontal axis plots $1 - p_B$, the probability that the agent rejects if the state is bad; the vertical axis plots p_G, the probability that the agent accepts if the state is good. The choice set is $\{(p_G, p_B) \in [0,1]^2 : p_G \geq p_B\}$. For better visibility, only a subset is plotted here. Straight lines represent indifference curves of a Bayesian decision maker. Curved lines are iso-cost functions. The solid, black arrows indicate the substitution effect. The dashed, red arrows indicate the stakes effect. Panel B. Contours of the posterior beliefs $P(s = G|participate)$ (dashed red lines) and $P(s = G|abstain)$ (solid black lines) for prior $\mu = 0.5$. Both posteriors increase towards the upper right.

such as point B. The agent is now more tolerant of false positives and takes greater care to avoid false negatives. He acquires a different *kind* of information.

An increase in m not only changes the relative cost of false negatives and false positives, it also changes the total stakes of this decision. Hence, the agent may choose to spend a different *amount* of resources on information acquisition. If the agent chooses to acquire a larger *amount* of information, his optimal bundle will move towards the upper right, for instance to a one such as point C.[38] This further decreases the incidence of false negatives, and counteracts the increase in false positives arising through the substitution effect. The stakes effect will not outweigh the substitution effect as long as the information cost function c has a negative cross-derivative. Posterior separability of the information cost function is a sufficient condition. (Posterior-separability is introduced in Caplin and Dean (2013b) and is satisfied, for instance, for Shannon mutual information costs.) The following proposition characterizes the total effect; the condition that the participation probabilities differ across states means that the agent bases his choice upon a positive amount of information, rather than on his prior alone.

[38]Depending on parameters, the stakes effect may have the opposite direction.

Proposition 1. *Consider an increase in the incentive from m to m', with $m' > m$. Let (p_G, p_B) and (p'_G, p'_B) denote the associated optimal state-contingent participation probabilities. If, moreover, $p_G \neq p_B$ and $p'_G \neq p'_B$, then the following holds:*

(i) $p'_G > p_G$ and $p'_B > p_B$ if $\frac{\partial^2 c}{\partial p_G \partial p_B} < 0$ everywhere.

(ii) If c is posterior-separable, then $\frac{\partial^2 c}{\partial p_G \partial p_B} < 0$ everywhere.

Even if a higher incentive increases both false positive and false negative probabilities p_G and p_B, it is still possible for it to lead to decisions that are better in the sense that the agent has more extreme posterior beliefs both after participating and after rejecting. The following proposition shows that if c is posterior-separable, a higher incentive induces the agent to participate at a less extreme posterior. Equivalently, conditional on participation, the agent is more likely to regret his decision *ex post*.[39]

Proposition 2. *If c is posterior-separable and twice differentiable, and if $p_G \neq p_B$ and $p'_G \neq p'_B$, then $\frac{\partial}{\partial m}\gamma_{participate} < 0$ and $\frac{\partial}{\partial m}\gamma_{abstain} < 0$.*

Graphically, Panel B of Figure 3 displays iso-posterior curves for $\gamma_{participate}$ and $1-\gamma_{abstain}$. Both functions increase toward the upper right. If c is posterior-separable, Proposition 2 therefore bounds the stakes effect relative to the substitution effect.

4.3 Why Bayesian behavior may look worrisome to a third party

Suppose a third party observes how the agent described by this model responds to an increase in the incentive amount. She will notice three patterns, which may easily be misconstrued as evidence of irrational self-deception, despite the fact that they derive from optimization by a Bayes-rational decision maker.

First, the agent demands information about the transaction that he expects will more likely be favorable to participation. To see why, think of the decision maker as following a signal that either tells him to participate or to abstain. The *ex ante* probability of participation is then equal to the chance of receiving a "participate" signal. It is given by

$$p = P(participate) = \mu p_G + (1-\mu) p_B \tag{2}$$

[39] Posterior separability implies that local variations in the prior do not change the agent's optimal distribution of posterior beliefs. Formally, c is posterior separable if it can be written as follows. Let $p = \mu p_G + (1-\mu)p_B$ the unconditional participation probability, and let $\gamma_{participate} = \frac{p_G \mu}{p}$ and $\gamma_{abstain} = \frac{(1-p_G)(1-\mu)}{1-p}$ be the agent's posterior belief about the event $\{s = G\}$ if he has observed a signal that makes him participate and abstain, respectively. c is *posterior separable* if it can be written as $c(p_G, p_B) = -h(\mu) + ph(\gamma_{participate}) + (1-p)h(\gamma_{abstain})$ for some strictly convex function $h : [0,1] \to \mathbb{R}$. c is the Shannon mutual information cost function if h is the negative of the binary entropy function, $h(x) = x\log(x) + (1-x)\log(1-x)$.

According to Proposition 1 (i), higher incentives lead to an increase in both p_G and p_B, and therefore to an increase in the *ex ante* probability with which the optimal information structure recommends participation.

Second, because the higher incentive induces a different optimal choice of information, the agent will have different beliefs about what the deliverable entails. Specifically, as the incentive increases, so does the chance that the agent's posterior beliefs exceed his prior. This is also a consequence of equation (2) and the fact that p_G and p_B increase with the incentive.

Third, the observer will notice that as the incentive for participation increases, a larger fraction of people who decided to participate *ex post* regret the decision. The reason is given by Proposition 2. According to that result, higher incentives induce subjects to participate based on lower posterior beliefs that the state is good, and that posterior is precisely the chance that the agent will *not* regret his decision *ex post* .

Through these three channels, Bayes-optimal behavior is therefore consistent with, for instance, the hypothesis by the American Society for Reproductive Medicine (2007) that "the higher the payment, the greater the possibility that women will discount risks [of providing oocytes]."

4.4 Welfare implications

Whether and when a social planner should be concerned with incentive effects in an environment of costly information acquisition depends on both the planner's welfare objective and the extent to which people empirically conform to Bayesian rationality.

Rational agents. First, consider the case of fully rational agents. In this case, introducing or increasing incentives to induce participation in a transaction cannot make people worse off *ex ante*. Hence, incentives are no cause for concern under a standard welfare economic perspective. But as incentives rise, so does the false positive probability, and hence the number of people who *ex post* regret participating. Therefore, introducing or raising incentives will generally not be an *ex post* Pareto improvement, even if the recipient of the transacted good unambiguously benefits. There are two welfare objectives under which these *ex post* effects are potentially concerning.

First, members of the general population (Cappelen et al., 2013; Andreoni et al., 2015) and professional ethicists (Satz, 2010; Kanbur, 2004) alike consider *ex post* undesirable outcomes worrisome in and of itself, in particular as they involve extreme outcomes such as destitution, or irreversible outcomes such as death. These concerns apply even if the actions that caused the outcome were entirely voluntary (Kanbur, 2004). Examples abound; policies as diverse as veteran service, personal bankruptcy laws, and emergency medical services mitigate adverse outcomes regardless of whether they result from voluntary decisions.[40]

Second, a policy that reduces some individuals' *ex post* welfare potentially raises inequality, even if it increases *ex ante* expected utility—particularly so if it predominantly affects the poor. In this case, the question whether to implement an incentive policy involves an equity-efficiency tradeoff.

Additionally, purely self-serving reasons (such as concerns about re-election) may render a politician wary of a policy that may cause significant *ex post* regret, even if it is *ex ante* beneficial.[41]

Irrational agents. If introducing or raising incentives leads to irrational reactions, then people who participate when a rational individual would have abstained may suffer from the policy even from an *ex ante* point of view. In this case, voluntary transactions are no longer Pareto improvements, and standard welfare economic arguments no longer imply they should necessarily be supported. Whether and by how much incentives skew the acquisition and interpretation of information about a transaction in an irrational manner is an empirical question. The experiment in Section 5 provides a preliminary answer; the observed deviations from rationality are minor.

4.5 Interpretation and extensions

Scope of the model. The predictions of this model apply as long as individuals choose to acquire *incomplete* information. This raises the question whether they still apply with high stakes decisions, such as organ donation. The answer depends not on the absolute magnitude of the stakes of the decision, but on how that magnitude compares to the information acquisition costs, as can be seen by dividing equation (1) by λ. Therefore, the predictions of the model apply to both the limited-stakes experiments in this paper, and to highly consequential decisions in which fully comprehensive information is extremely expensive to acquire, such as kidney donation. (To illustrate, most donors do not acquire a doctorate in nephrology.) The model predictions do not apply in cases where information acquisition is either prohibitively expensive, or close to costless relative to the stakes of the decision.[42]

[40] Of course, there exist additional rationales for each of these policies.

[41] To illustrate, the documentary Eggsploitation (2011) criticizes incentives for egg donation based on *ex post* unfavorable outcomes.

[42] Ambuehl, Ockenfels and Stewart (2017) explore this mechanism in depth, showing both theoretically and experimentally that agents with higher marginal costs of information acquisition respond more strongly to changes in incentive amounts. Higher incentives therefore lead to a disproportionate selection of subjects with high costs of information acquisition who choose to participate based on a weaker understanding of the transaction.

Standard effects of incentives. In this model, supply responds to incentives *only* because it leads to differential information acquisition. A perfectly informed decision maker, however, would always abstain in the bad state and participate in the good state as long as $-\pi_G < m < -\pi_B$, leading to a non-responsive supply. In this sense, the model abstracts from the fact that a higher incentive will often increase participation simply because it exceeds the reservation price of a larger number of people. Appendix B.1 allows for both effects and shows that results remain essentially unchanged.

Learning about magnitudes. This model assumes that the state-dependent payoffs π_G and π_B are fixed and known; learning only concerns the probability of the state. In any applied setting, by contrast, people likely also acquire information about the distribution of the magnitude of the (dis)utility associated with providing the deliverable. Appendix B.1 extends the model to include uncertainty about magnitudes by allowing the unknown state s to take any value on the real line. The qualitative conclusions remain unchanged.

Interpreting the insect experiment. As argued in Section 4.3, the model predicts behavior similar to that observed in the insect experiment: Higher incentives skew subjects' information acquisition towards more encouraging sources and away from discouraging sources in a way that affects participation decisions, and they alter beliefs about the deliverable of the transaction.[43]

Since that experiment is not designed to be a test of the theory, a more detailed mapping is speculative.[44] Two salient differences between that experiment and the model are: First, subjects are required to choose one of only two available information sources (although they may have leeway in how to interpret each video). Second, information in that experiment may not only change beliefs about the experience of eating insects, it may also affect that experience directly.

An explicit test of the model is in the experiment in the following section.

[43] In particular, the model predicts that with higher incentives, subjects become more interested in the encouraging video, under the following assumption: The encouraging video is more likely to increase beliefs than the discouraging one, in every state of the world. Formally, the encouraging and discouraging videos correspond to pairs (p_G^+, p_B^+) and (p_G^-, p_B^-) of state-dependent participation probabilities, with $1 > p_G^+ > p_G^- > 0$ and $1 > p_B^+ > p_B^- > 0$. Note that this assumption is *not* a statement about how convincing subjects will find the video after having watched it. For instance, the assumption does *not* rule out the possibility of someone choosing the encouraging video and finding it so unconvincing that she forms even more pessimistic beliefs about insect eating than if she had watched the discouraging video. In fact, this is precisely what would happen with positive probability to a Bayesian if the encouraging video decreases the false negative rate $1 - p_G$ sufficiently relative to the increase in the false positive rate p_B, since the pessimistic realization of the Bayesian posterior, $P(s = G|abstain) = \frac{\mu(1-p_G)}{\mu(1-p_G)+(1-\mu)(1-p_B)}$ is decreasing in p_G and increasing in p_B.

[44] Specifically, barring additional assumptions, the model does not make a prediction about how incentives will affect average posterior reservation prices. By the law of iterated expectations, the distribution of posterior beliefs is a mean-preserving spread of the prior, and the type of spread depends on the incentive. Hence, the effect of the incentive on mean posterior beliefs depends on the shape of the mapping of posterior beliefs into reservation prices.

5 Incentives and information in a stylized experiment

In this section I present an experiment that conceptually replicates the experiment in Section 3, that is sufficiently stylized to allow for an explicit comparison between empirical behavior and Bayesian rationality, and that permits a rigorous test of the model in Section 4. The design excludes explanations that might apply to the insect experiment but do not have an interpretation within the primitives of the model. For instance, the only good involved is money, so that the treatments can only affect *beliefs* about outcomes rather than the outcomes *per se*. (In the insect experiment, by contrast, it is conceivable that watching an unpleasant video about insects may make the experience of ingesting them more revolting.)

5.1 Design

The structure of this experiment parallels the first four stages of the experiment in Section 3. The difference is that subjects are not incentivized to eat insects, but instead to take an unattractive gamble. Someone who takes it and gets lucky loses nothing; someone who gets unlucky loses \$3.50. Each happens with known prior probability 0.5.

The experiment follows a 2×2 design. Each subject participates in each condition exactly once, in random order.[45] The first dimension varies the incentive for participating in the gamble, which is either high (\$3) or low (\$0.50). Hence, subjects in the low incentive condition decide whether to take a win \$0.50 / lose \$3 gamble, and those in the high incentive condition decide over a win \$3 / lose \$0.50 gamble; all see their options presented in this way.[46]

Before subjects decide whether to take the gamble, they can inform themselves whether they will win or lose. They do so by examining a picture of 450 randomly arranged letters as in Figure 4. Subjects know that if the lottery leads to a net gain, the picture contains 50 letters of G and 40 letters of B (for "good" and "bad", respectively), and that these numbers are reversed if the lottery leads to a loss. There is no time constraint. Hence, somebody willing to put in the time and effort to count the letters can know with certainty whether taking the gamble will lead to a gain or a loss.[47]

The second treatment dimension varies the point in time subjects learn the incentive for taking the gamble. In the *incentive first* condition they first learns the incentive, and then view the picture. In the *picture first* condition, this order is reversed. Before examining the picture,

[45]In addition to the four main treatments, each subject participates in two ancillary treatments that differ from the main treatments only regarding how much money can be lost or won. See Appendix C.2 for details.

[46]This representation of payoffs, as opposed to presenting the incentive of \$3 or \$0.5 separately from the win \$0 / lose \$3.50 gamble, aims to minimize confusion.

[47]For clarity, each picture was presented within a colored frame, and referred to by that color. For each subject these colors were randomly assigned to rounds.

subjects only learn that the net upside of the bet will be \$3 or \$0.5 with equal chance, and that the net downside will be \$3 or \$0.5 with equal chance. Hence, incentives may influence her information acquisition in the *incentive first* but not in the *picture first* condition.[48]

After deciding whether to take the gamble, subjects reveal their subjective posterior belief that they have seen a good picture by selecting one of 12 bins, corresponding to 0, 5, 15, 25, ..., 85, 95, or 100 percent certainty that the state in that round was good. This data permits a comparison between objective and subjective beliefs, and thus a test of Bayesian rationality. Truthtelling is incentivized by the binarized scoring rule (Hossain and Okui, 2013).[49] At any point during the belief elicitation stage, subjects can return to the previous stage of the experiment, for instance if the belief elicitation leads them to reassess their decision to take the gamble.[50]

Relation to the model. The *incentive first* treatment directly maps to the model in Section 4 with parameters $m \in \{0.5, 3\}$, $\pi_G = 0$, $\pi_B = -3.5$ and $\mu = 0.5$. The experiment does *not* induce the information cost function c. Instead, it is naturally given by subjects' disutility from tediously examining the pictures of scrambled letters and their opportunity cost of time.

To see how incentives may affect information acquisition in the *incentives first* condition, consider the example of a subject with a sequential information acquisition strategy like those displayed in Figure 2. She scans through the picture, and keeps track of the number of Gs and Bs she has encountered. She does so until she has either seen two more Gs than Bs, in which case she takes the gamble; or until she has seen six more Bs than Gs, in which case she rejects. This subject's criterion for accepting the gamble is less stringent and thus more likely to be satisfied by chance than that for rejecting the gamble. Hence, she will more likely commit a false positive error than a false negative error. By choosing different criteria of when to stop searching and accept or reject the gamble, she can change the false positive and false negative probabilities. Such a decision rule can depend on the incentive amount only in the *incentive first* but not in the *picture first* condition.

[48] Caplin and Dean (2013a,b) use a similar method of presenting information. Babcock and Loewenstein (1997) and Gneezy et al. (2015) use a similar treatment variation to prevent the dependence of information acquisition and interpretation on situational factors.

[49] This rule is incentive compatible regardless of the shape of a subject's utility for money, as long as preferences are linear in probabilities. Briefly, an agent is incentivized for his report r about his subjective probability of an event A as follows: The computer independently draws probability q from a uniform distribution. If $q > r$, the agent receives a prize with probability q. If $q \leq r$, the agent receives the prize if event A occurs. Since subjective probabilities are elicited in bins, I use the midpoint of each bin to determine payments; subjects were informed of this fact.

[50] For readers nonetheless concerned that elicited beliefs are merely an *ex post* rationalization of the betting decision, I test and reject this hypothesis in Appendix C.1.

Payment. Participants are paid for one randomly selected decision of one randomly selected round and thus have an incentive to reveal their genuine preferences in each decision. Subjects are aware that they will be paid according to a betting decision with 80% probability, and according to a belief elicitation decision with 20% probability. The larger weight placed on the betting decision serves to increase the effect of the incentive condition on information acquisition. Losses are discounted from a completion payment of \$6; gains are added. On average, subjects took 33 minutes to complete the study.[51]

Implementation. I conducted this experiment on the Amazon Mechanical Turk online labor market with 450 subjects in March and October of 2015 and with an additional 503 subjects in April of 2016.[52] Laborers on that platform typically earn an hourly wage of around \$5 (Horton, Rand and Zeckhauser, 2011; Mason and Suri, 2012). All instructions were presented on screen. Subjects were allowed to proceed only if they answered each of eleven simultaneous True/False statements about the instructions correctly.[53]

Each subject participated in each treatment once, in individually randomized order. For each subject and each decision, a state of the world was drawn according to the uniform prior probability, and a new picture of scrambled letters corresponding to that state was randomly generated. To prevent subjects in the *picture first* condition from allowing incentives to skew their attention allocation, subjects could not return to the picture once they had decided to continue.[54]

5.2 Analysis

I first test propositions 1 and 2 by analyzing the effect of incentives on state-contingent choice probabilities and distribution of posterior beliefs, respectively. I then test for rationality by comparing objective to subjective posterior beliefs. Throughout, I use the *picture first* treatment as a control. This allows me to isolate the effect of the incentive amount on information acquisition alone.

[51] The time spent examining a picture is right-skewed, with a mean of 59 seconds per picture, and a median of 30 seconds. Response times are also highly dispersed, with a standard deviation of 87 seconds.

[52] Data on subjective beliefs are available for the April session only. The former sessions employed a version of the quadratic scoring rule which was unsuitable due to its sensitivity to risk non-neutrality.

[53] If a subjects answers incorrectly, she only learns that at least one of the statements is marked incorrectly. Hence, it is highly unlikely that participants can successfully complete this task by chance. The instructions and test questions are reproduced in Appendix D.

[54] Subjects could not use a text editor to automatically count the letters because they were presented in a picture format (HTML5 Canvas). In principle, subjects could have taken screenshots to refer back to the picture after incentives are revealed in the *picture first* condition. If they did, my results would underestimate the effect of endogenous attention allocation.

```
w q t f B G r p o w t o r q p r q d s s s d w y G p o G t d
f s B B f w B p q G o d t o r t q r t q t y B o t B o f o t
s s r r s s s d p s r B G B w f w t t r d q f y B w B f w s
d w f f p s q o y y w d w r B p G s G p s q o w r G o G p o
s y q B w d r w t G f r f p G p B w o r y q w r o G r p y y
q y r f w f q r G t w f y d f G y y p B f t r r f o G G t s
f y q o y B B r y r p d B o t f d B t y p p o r q y d G s p
p f y t B t y y y t d G o G B s p B p s o f s B w r y G r t
r G y G B r B r r w q G G B w t q d G f w w q y q t B t f G
y G f B f d G d w f r y t s p t f t r d w d d r t G G s d r
q w y w G G w t B G f s o B r r G y w p B w t f s p G p w q
G o B t G w B B G s r t f t s y f t y d s p q p t r f y p f
G r d p s o t o w o d G f G r w q y B G d q s q s B y B p y
s t q t d G o w G p G t d t o q G q t t f d t B q o y o d w
s G B f d B G w G q o r w o y f s p B d o w o s r p f f G q
```

Figure 4: Presentation of information about the state. In case the subject will win upon taking the gamble, the picture has 50 letters G and 40 letters B. In case the subject will lose, it has 40 letters G and 50 letters B.

State-contingent participation probabilities. Table 4 displays the fraction of subjects who take the gamble if the state is bad (Panel A1) and if the state is good (Panel A2), separately for each information condition. These numbers are the fractions of false positive and correct positive choices, and allow for a test of Proposition 1, .

Focusing on Panel A1, we see that the raising the incentive increases participation in the bad state by 44.81 percentage points in the *incentive first* condition, though by a significantly smaller 32.44 percentage points in the *picture first* condition. Hence, through its effect on information acquisition, the rise in the incentive increases the false positive rate by 12.38 percentage points.

Panel A2 shows a similar result. With a higher incentive, participation in the good state increases by 53.27 percentage points in the *incentive first* condition, though by a significantly smaller 45.21 percentage points in the *picture first* condition. Hence, through its effect on information acquisition, the rise in the incentive lowers the false negative rate by 8.05 percentage points.

Averaging across the two states yields the supply curves, as shown in Panel A3, and plotted in Panel A of Figure 5. The supply response to the increased incentive is 9.44 percentage points larger when information acquisition can depend on the incentive (the *incentive first* condition) than when it cannot (the *picture first* condition). This replicates the corresponding result from the insect experiment.[55]

[55]In the current experiment, the model unambiguously predicts a stronger supply response in the *incentive first* than in the *picture first* condition (see Appendix B.2 for a formal derivation). By contrast, the model is merely consistent with the corresponding result in the insect experiment, but would also be consistent with the opposite. The reason for this is that, in the insect experiment, subjects in the *no video* condition have access to

A. State-contingent choice probabilities $P(\text{action}|\text{state})$

State	A1. False positives Bad only			A2. Correct positives Good only			A3. Supply curves Both		
Incentive	\$0.50	\$3	*Difference*	\$0.50	\$3	*Difference*	\$0.50	\$3	*Difference*
Condition									
Incentive first	7.93	52.75	44.81***	36.29	89.56	53.27***	22.11	71.15	49.04***
	(1.24)	(2.39)	(2.68)	(2.21)	(1.35)	(2.58)	(1.35)	(1.50)	(2.13)
Picture first	12.29	44.74	32.44***	41.87	87.08	45.21***	26.89	66.50	39.61***
	(1.60)	(2.55)	(3.00)	(2.55)	(1.64)	(3.05)	(1.59)	(1.68)	(2.40)
Difference	-4.36**	8.02**	12.38***	-5.58*	2.47	8.05**	-4.78**	4.66**	9.44***
	(1.90)	(3.21)	(3.77)	(3.01)	(2.00)	(3.66)	(1.86)	(2.14)	(2.89)

B. Posterior probabilities $P(\text{state}|\text{action})$

Action	B1. Bet taken			B2. Bet refused		
Incentive	\$0.50	\$3	*Difference*	\$0.50	\$3	*Difference*
Condition						
Incentive first	82.06	62.93	-19.13***	40.90	18.10	-22.80***
	(2.64)	(1.89)	(3.20)	(1.81)	(2.27)	(2.79)
Picture first	77.96	66.40	-11.56***	39.64	18.67	-20.98***
	(2.58)	(1.89)	(3.10)	(1.86)	(2.16)	(2.76)
Difference	4.10	-3.46	-7.57*	1.26	-0.56	-1.82
	(3.60)	(2.56)	(4.53)	(2.54)	(0.30)	(3.93)

Table 4: Panels A1 - A3 display (state-contingent) participation probabilities $P(\text{action}|\text{state})$. A1 and A2 separately show participation rates in the bad and good states, respectively. A3 pools across states. They feature 1923, 1889, and 3502 observations from 893, 892, and 953 subjects, respectively. Panels B1 and B2 display posteriors $P(\text{state}|\text{action})$, based on 3502 observations from 953 subjects. In each regression that includes both states, exactly half the total weight is given to good-state observations, and half to bad-state observations. Standard errors are clustered by subject. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Asterisks are suppressed for levels.

Objective posteriors. To test Proposition 2, Panels B1 and B2 of Table 4 show, for each treatment, the fraction of subjects who had seen a good-state picture, conditional on the action they have taken. These numbers are estimates of the Bayesian posterior $P(s = G|action)$. They exactly coincide with the result one would obtain by applying Bayes' law to the state-contingent participation probabilities in Panel A.

a different opportunity set of information structures than those in the *video* condition, whereas in the present experiment, subjects in both the *incentive first* and *picture first* condition face the same opportunity set of information structures.

Focusing on subjects who chose to gamble (Panel B1) in the *picture first* condition, we see that the posterior at which subjects decide to participate is 11.56 percentage points lower if the incentive is high. In the *incentive first* condition, by contrast, this number is 19.13. The 7.57 percentage point difference in these effect sizes confirms the prediction of Proposition 2—with higher incentives, subjects participate at lower posteriors because they acquire different information.

By contrast, for subjects who decided against the gamble (Panel B2), the difference between the *incentive first* and *picture first* conditions is considerably attenuated and statistically insignificant. In this experiment, subjects' thresholds for accepting the gamble respond more to changes in incentives than those for rejecting it. While this comparative static is not predicted by the model, it is consistent with the literature on opportunity cost neglect (Frederick et al., 2009).

Subjective posterior beliefs and rationality. Finally, I test for Bayesian rationality by comparing elicited posteriors to an objective benchmark, in the following two ways.

First, I examine whether elicited posterior beliefs satisfy the law of iterated expectations. Formally, this law states that a Bayesian's expected posterior must equal his prior. Intuitively, a Bayesian may not expect to become more optimistic, on average, after observing information—if he did, he should be more optimistic already. Because this law holds regardless of the information structure a Bayesian chooses to observe, it is well applicable in the current experiment.

Panel B of Figure 5 displays mean elicited posteriors in the *incentive first* condition.[56] It exceeds the prior of 50% by 4.53 percentage points (s.e. 1.35) if the incentive is high, and falls short of it by 3.55 percentage points (s.e. 1.25) if the incentive is low. Hence, an increase in the incentive makes subjects more optimistic in a fashion that violates the law of iterated expectations and thus Bayesian rationality. This effect, however, does not significantly differ across information conditions.[57] Hence, while higher incentives do make subjects systematically more optimistic, this is not because of the way they affect information acquisition. Moreover, while the magnitude of the deviation from rationality is statistically significant, its magnitude is limited.

Second, I compare elicited and Bayesian posteriors on a disaggregated level. A subject's reported posterior p coincides with the Bayesian posterior if the objective probability that this

[56] 5.47% of participants revised their decision about the bet during the belief elicitation stage in at least one round. They did so infrequently; only 1.01% of all decisions are changed. Due to a coding error, these decisions were not recorded in the October 2015 session; the reported numbers are based on the remaining 658 participants.

[57] The respective mean posteriors in the *picture first* condition are 46.48% (s.e. 1.32) if the incentive is low, and 53.44% (s.e. 1.31) if it is high.

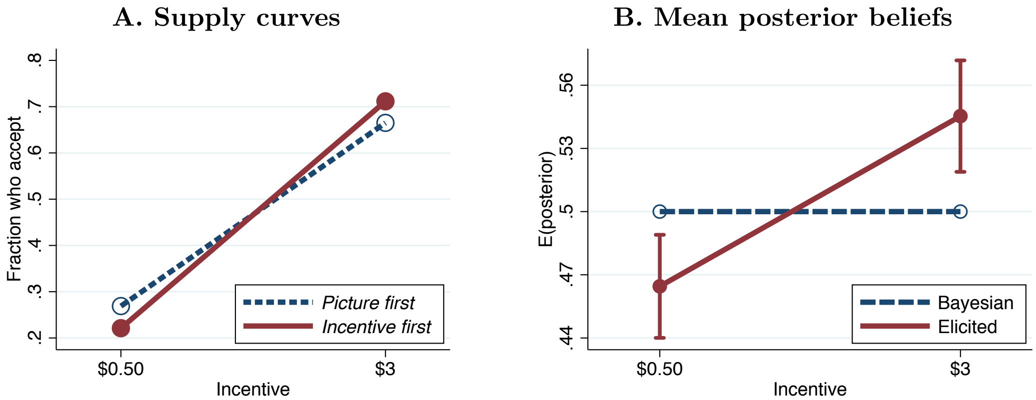


Figure 5: Choice probabilities and posterior beliefs that the state is good. Panel A displays the fraction of subjects accepting the gamble by treatment condition. Panel B shows mean elicited posterior beliefs in the *incentive first* condition and compares them to the Bayesian benchmark.

subject has seen a good-state picture is p. To operationalize this idea empirically, I fix a level of elicited posterior belief p and count the number of times the state has actually been good amongst all subjects who report that posterior.

Figure 6 shows that elicited posteriors track objective posteriors surprisingly closely (data are averaged across all treatments). Amongst subjects who, for instance, report subjective posteriors of 65%, I find 67.7% actually saw a good-state picture; and a similar result holds for many other elicited posteriors. Deviations rarely exceed a handful of percentage points and statistically significant differences are rare.[58] This extent of rationality is particularly striking in light of previous research on belief updating with exogenously given information, both in the heuristics-and-biases tradition (Tversky and Kahneman, 1974) and in the man-as-intuitive-statistician tradition (Peterson and Beach, 1967).[59]

[58]The fact that elicited and objective posteriors are close even though different incentives substantially change their distribution suggests that subjects are well aware of the effect of incentives on information acquisition and beliefs. In the insect experiment, by contrast, subjects are unable to predict how incentives affect reservation prices when endogenous information acquisition is possible. While there are numerous differences between the two experiments, a plausible candidate explanation for the difference is the following. In the present experiment, a subject knows the total number of letters in a picture, and is therefore aware of the fraction of potentially available information she has consulted. In the insect experiment, by contrast, it is less plausible that subjects have an idea about how much could potentially be known about insects as food. It is thus difficult to know what fraction of the total available information one has consulted, what reasoning one would have engaged in in the absence of that information, and how one's beliefs would change upon the arrival of information one does not yet have. Consequently, in the insect experiment, it is likely harder to be well-aware of the precise effect of incentives on one's beliefs.

[59]A possible explanation for this difference is that typical belief updating experiments provide subjects with numerical values describing a vector of state-contingent error probabilities. No such description is needed in the present experiment. Deviations from Bayesianism in the literature might be due to difficulties with internalizing numerical values, rather than with a failure to manipulate values correctly once subjects have internalized them.

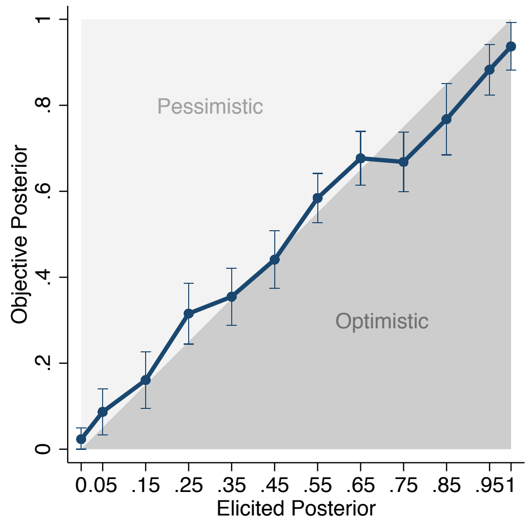


Figure 6: Elicited and objective posteriors. This figure plots estimates of the Bayesian posterior for each level of elicited posterior. Whiskers indicate 95% confidence intervals. Standard errors are clustered by subject.

Hence, while deviations from Bayesian rationality in this experiment with endogenous information acquisition take the direction that ethicists and policy makers worry about, these deviations are limited in magnitude. In particular, within the context of this experiment, a higher incentive does make subjects *ex ante* better off, in spite of the observed deviations from rationality.

6 Policy implications and further applications

Policies for transactions with currently restricted incentives. If one endorses a welfare functional that prescribes limiting the effects of incentives on information acquisition described in this paper, what are appropriate policies to do so?[60]

Two classes of policies can curtail the effects of incentives documented in this paper without restricting incentives. One class focuses on increasing the information people possess when deciding whether or not to enter a transaction, while the other class deals with compensating undesirable outcomes *ex post*. Information can be increased in two ways. First, policy can simply obligate people to be well-informed before they participate, for instance through stringent

[60]There are concerns with incentives for certain transactions that do not relate to the acquisition and interpretation of information. An important idea is that incentives may hurt individuals with time-inconsistent preferences (Frederick, Loewenstein and o'Donoghue, 2002). Such concerns can be addressed by cooling-off periods (Becker and Elias, 2007) or by making the disbursement of incentive payments coincide with the incidence of the utility costs of participating in the transaction over time.

informed consent requirements.[61] In some cases, such as gestational surrogacy, participation can be limited to those who have undergone a similar experience before, and can thus better predict the consequences of participation. For example, commercial surrogate motherhood is legal in Russia, but only for women who have a child of their own (Svitnev, 2010). Second, policy can decrease the information acquisition costs, for instance by making information more easily accessible.

The second approach focuses on compensating participants for *ex post* undesirable outcomes. In principle, this may reduce the difficult equity-efficiency tradeoff outlined in Section 4.4 into a simpler question of efficiency maximization. Such a policy, however, faces at least three obstacles. First, *ex post* undesirable outcomes are the reason why subjects acquire costly information about a transaction. A policy that insures against such outcomes therefore dilutes incentives for information acquisition, a form of moral hazard discussed below. Second, welfare losses from undesirable *ex post* outcomes may not be perfectly observable, and incentivizing truthful revelation is hardly possible after the fact. Hence, determining adequate compensation and eligibility may be difficult.[62] Third, a significant part of the ethics literature maintains that it is fundamentally impossible to compensate people for the loss of certain goods such as bodily integrity (Anderson, 1995; Sandel, 2012; Sen, 1985, 1999; Nussbaum, 1995).

Further applications *Informational moral hazard.* Insuring participants against adverse outcomes is a frequently discussed policy in domains such as living kidney donation (Rosenberg, 2015a; Eyal et al., 2014). This paper suggests the potential side effect of insurance increasing the fraction of participants who choose to participate but later regret this decision. This is because a Bayes-rational agent acquires costly information in order to avoid false positives. Insurance against undesirable outcomes decreases the cost of false positives, and thus the incentive for information acquisition.[63] Unlike moral hazard that arises, for instance, when car insurance leads to careless driving, informational moral hazard can be mitigated by providing better information to the insured.

[61] The ethics literature supports such requirements (Faden and Beauchamp, 1986). Also the respondents in Ambuehl and Ockenfels (2017) overwhelmingly support interventions to maintain adherence to the principle of informed consent. In the specific domain of living kidney donation, current informed consent practices are imperfect (Thiessen et al., 2013). Informed consent documents often do not supply all required pieces of information, and are highly variable in items as crucial as complications related to surgery, health problems following surgery, and payment for routine follow-up care. Moreover, informed consent for living kidney donors is currently thin on psychological and emotional preparedness. Prospective living donors spend 45 minutes with a social worker or other mental health professional and equal time with a living donor advocate (livingdonor101.com, 2013).

[62] See Mitchell and Moro (2006) for a related issue regarding compensating those who lose from trade liberalization.

[63] A related literature starting with Doherty and Thistle (1996) considers how endogenous information acquisition interacts with adverse selection. That literature abstracts from the kind of moral hazard that arises due to endogenous information acquisition.

Incentives and expert opinion. Most fundamentally, this paper shows that if a person has incentives for entertaining certain beliefs, then that person will adjust his or her acquisition and interpretation of information accordingly. This finding has implications for many situations in which experts charged with acquiring and interpreting information are subject to incentives. In the domain of personal finance, for instance, Linnainmaa, Melzer and Previtero (2016) present evidence that financial advisors' personal investment portfolios are often shaped by the same misguided investment strategies they recommend to their clients. For advisers who are incentivized to promote particular products and services, the present paper suggests that they will search for and interpret information in a way that supports giving this advice. Because this is driven by information acquisition, and potentially entirely rational, incentivized experts will harbor correspondingly skewed beliefs themselves, and follow their own biased advice. In a similar vein, this paper suggests that lobbying does not merely change how politicians vote; through its power to influence how people acquire and interpret information, incentives also re-shape their sincere beliefs about policies. The same mechanism provides further explanation as to why pharmaceutical gifts to doctors may be effective (Campbell et al., 2007; Morgan et al., 2006; Wazana, 2000). It also explains why even perfectly honest accountants may find it in their interest to allocate their scarce resources such as time and funds in a way that ultimately leads to skewed reporting (Bazerman et al., 2002).

7 Conclusion

This paper shows that incentives skew how people inform themselves about a transaction, and thus reshapes their belief about what the the deliverable entails, in a way that causally influences the decision they ultimately make. By shaping people's beliefs about their choice options, incentives thus genuinely persuade.

These mechanisms apply whenever incentives coincide with costly information acquisition, and are thus potentially relevant in many subfields of economics. The finding has a particular relevance regarding the many laws and regulations that restrict the nature and extent of incentives to participate in transactions like organ donation, gestational surrogacy, and medical trial participation, among others. An important motivation for such laws is the idea that incentives lead to poor decision making. This paper shows that incentives indeed change behavior in a way which, at first glance, may appear worrisome. A model of costly information acquisition shows, however, that even perfectly Bayes-rational agents would act in the same fashion. The model thus advances two objectives. First, it clarifies the conditions on the welfare functional under which rational behavior may or may not justifiably be regarded as concerning. The extent to which one considers the *ex post* distribution of outcomes welfare-relevant plays a crucial role. Second, it precisely delineates the type of empirical findings that are, and are not, evidence for bad decision making.

There are reasons other than those addressed in this paper for which ethicists disapprove of incentives for particular transactions. Most closely related to the economics literature is the concern that incentives may hurt individuals with time-inconsistent preferences (Frederick, Loewenstein and o'Donoghue, 2002). Such concerns are important, and can be addressed separately from those studied in this paper.[64] Conversely, incentives do not always raise concerns, even when they coincide with costly information acquisition, such that the mechanisms identified in this paper potentially apply.[65] It appears that concerns are more frequent for transactions in which there is a chance of significantly damaging an individual's well-being, with consequences that may be very difficult to reverse. In other cases, potential negative effects of participation may be limited, and, accordingly, so are ethical concerns.

Most broadly, this paper bridges a gap between economics on the one hand, and the applied ethics and policy literatures on the other. Through standard economic methodology, this paper informs a prominent and highly influential, but vaguely formulated and largely untested hypothesis about the effects of incentives. Generally, using the powerful toolbox of economics to examine moral intuitions and the behavioral assumptions on which they rely is an important direction for future research, for otherwise, our laws would be based on intuition and speculation alone.

[64] Concerns about time-inconsistent preferences can be addressed, for instance, by cooling-off periods (Becker and Elias, 2007) or by timing the disbursement of incentive payments such that they coincide with the incidence of the utility costs of participating.

[65] One example is the decision to accept a new job in exchange for a higher salary.

References

Ambuehl, Sandro and Axel Ockenfels, "The Ethics of Incentivizing the Uninformed. A Vignette Study," *The American Economic Review Papers and Proceedings*, 2017, *107* (5), 91–95.

_ , _ , **and Colin Stewart**, "For They Know Not What They Do. Selection Effects of Incentives with Costly Information Acquisition.," *unpublished*, 2017.

_ , **Muriel Niederle, and Alvin E. Roth**, "More Money, More Problems? Can High Pay be Coercive and Repugnant?," *American Economic Review, Papers & Proceedings*, 2015, *105* (5), 357–60.

American Society for Reproductive Medicine, "Financial compensation of oocyte donors," *Fertility and Sterility*, 2007, *88* (2), 305–309.

Anderson, Elizabeth, *Value in ethics and economics*, Harvard University Press, 1995.

Andreoni, James, Deniz Aydin, Blake Barton, B. Douglas Bernheim, and Jeffrey Naecker, "When Fair Isn't Fair: Sophisticated Time Inconsistency in Social Preferences," *unpublished manuscript*, 2015.

Ariely, Dan, George Loewenstein, and Drazen Prelec, "'Coherent Arbitrariness': Stable Demand Curves Without Stable Preferences," *The Quarterly Journal of Economics*, 2003, *118* (1), 73–105.

Babcock, Linda and George Loewenstein, "Explaining bargaining impasse: The role of self-serving biases," *The Journal of Economic Perspectives*, 1997, *11* (1), 109–126.

Basu, Kaushik, "The economics and law of sexual harassment in the workplace," *The Journal of Economic Perspectives*, 2003, *17* (3), 141–157.

_ , "Coercion, Contract and the Limits of the Market," *Social Choice and Welfare*, 2007, *29* (4), 559–579.

Bazerman, Max H, George Loewenstein, and Don A Moore, "Why good accountants do bad audits," *Harvard business review*, 2002, *80* (11), 96–103.

Beavers, Kimberly L, Robert S Sandler, Jeffrey H Fair, Mark W Johnson, and Roshan Shrestha, "The living donor experience: donor health assessment and outcomes after living donor liver transplantation," *Liver transplantation*, 2001, *7* (11), 943–947.

Becker, Gary S and Julio J Elias, "Introducing incentives in the market for live and cadaveric organ donations," *The Journal of Economic Perspectives*, 2007, *21* (3), 3–24.

Bénabou, Roland, "The Economics of Motivated Beliefs," *Revue d'économie politique*, 2015, *125* (5), 665–685.

_ **and Jean Tirole**, "Self-confidence and personal motivation," *Quarterly Journal of Economics*, 2002, *117* (3), 871–915.

_ **and** _ , "Incentives and prosocial behavior," *The American Economic Review*, 2006, *96* (5), 1652–1678.

_ **and** _ , "Identity, morals, and taboos: Beliefs as assets," *The Quarterly Journal of Economics*, 2011, *126* (2), 805–855.

_ **and** _ , "Mindful Economics: The Production, Consumption, and Value of Beliefs," *The Journal of Economic Perspectives*, 2016, *30* (3), 141–164.

Benoît, Jean-Pierre and Juan Dubra, "Apparent overconfidence," *Econometrica*, 2011, *79* (5), 1591–1625.

Boo, Michael, Suzanna M van Walraven, Jeremy Chapman, Brian Lindberg, Alexander H Schmidt, Bronwen E Shaw, Galen E Switzer, Edward Yang, Torstein Egeland, and World Marrow Donor Association, "Remuneration of hematopoietic stem cell donors: principles and perspective of the World Marrow Donor Association," *Blood*, 2011, *117* (1), 21–25.

Campbell, Eric G, Russell L Gruen, James Mountford, Lawrence G Miller, Paul D Cleary, and David Blumenthal, "A national survey of physician–industry relationships," *New England Journal of Medicine*, 2007, *356* (17), 1742–1750.

Caplin, Andrew, "Measuring and Modeling Attention," *Annual Reviews of Economics*, 2016, *8*, 379–403.

_ **and John Leahy**, "Psychological expected utility theory and anticipatory feelings," *Quarterly Journal of Economics*, 2001, *116* (1), 55–79.

_ **and Mark Dean**, "Rational inattention and state dependent stochastic choice," *unpublished manuscript, New York University*, 2013.

_ **and** _ , "Rational Inattention, Entropy, and Choice: The Posterior-Based Approach," *unpublished manuscript, New York University*, 2013.

Cappelen, Alexander W, James Konow, Erik Ø Sørensen, and Bertil Tungodden, "Just luck: An experimental study of risk-taking and fairness," *The American Economic Review*, 2013, *103* (4), 1398–1413.

Center for Bioethics and Culture, "Eggsploitation," www.eggsploitation.com 2011.

Cragg, John G, "Some statistical models for limited dependent variables with application to the demand for durable goods," *Econometrica*, 1971, *39* (5), 829–844.

Cryder, Cynthia E., Alex John London, Kevin G. Volpp, and George Loewenstein, "Informative inducement: Study payment as a signal of risk," *Social Science & Medicine*, 2010, *70*, 455–464.

Danna, Daniela, "Report on prostitution laws in the European Union," *unpublished manuscript*, 2014.

Doherty, Neil A and Paul D Thistle, "Adverse selection with endogenous information in insurance markets," *Journal of Public Economics*, 1996, *63* (1), 83–102.

Dong, Diansheng and Harry M Kaiser, "Studying household purchasing and nonpurchasing behaviour for a frequently consumed commodity: two models," *Applied Economics*, 2008, *40* (15), 1941–1951.

_ , Chanjin Chung, and Harry M Kaiser, "Modelling milk purchasing behaviour with a panel data double-hurdle model," *Applied Economics*, 2004, *36* (8), 769–779.

Elias, Julio J, Nicola Lacetera, and Mario Macis, "Markets and Morals: An Experimental Survey Study," *PloS one*, 2015, *10* (6).

Elias, Julio, Nicola Lacetera, and Mario Macis, "Sacred Values? The Effect of Information on Attitudes Toward Payments for Human Organs," *American Economic Review, Papers & Proceedings*, 2015, *105* (5), 361–365.

Eliaz, Kfir and Ran Spiegler, "Can anticipatory feelings explain anomalous choices of information sources?," *Games and Economic Behavior*, 2006, *56*, 87–104.

Emanuel, Ezekiel J, "Ending concerns about undue inducement," *The Journal of Law, Medicine & Ethics*, 2004, *32* (1), 100–105.

Engel, Christoph and Peter G Moffatt, "dhreg, xtdhreg, and bootdhreg: Commands to implement double-hurdle regression," *Stata Journal*, 2014, *14* (4), 778–797.

Epley, Nicholas and Thomas Gilovich, "The Mechanics of Motivated Reasoning," *Journal of Economic Perspectives*, 2016, *30* (3), 133–40.

Eyal, Nir, Julio Frenk, Michele B. Goodwin, Lori Gruen, Gary R. Hall, Douglas W. Hanto, Frances Kissling, Ruth Macklin, Steven Pinker, Lloyd E. Ratner, Harold T. Shapiro, Peter Singer, Andrew W. Torrance, Robert D. Truog, and Robert M. Veatch, "An Open Letter to President Barack Obama, Secretary of Health and Human Services Sylvia Mathews Burwell, Attorney General Eric Holder and Leaders of Congress," 2014.

Faden, Ruth R and Tom L Beauchamp, *A history and theory of informed consent*, Oxford University Press, 1986.

Farley, Melissa, "Prostitution, Liberalism, and Slavery," *Logos: A Journal of Modern Society & Culture*, 2013, *46* (3).

Frederick, Shane, George Loewenstein, and Ted o'Donoghue, "Time discounting and time preference: A critical review," *Journal of Economic Literature*, 2002, *40* (2), 351–401.

_ , Nathan Novemsky, Jing Wang, Ravi Dhar, and Stephen Nowlis, "Opportunity cost neglect," *Journal of Consumer Research*, 2009, *36* (4), 553–561.

Frey, Bruno S and Reto Jegen, "Motivation Crowding Theory," *Journal of Economic Surveys*, 2001, *15* (5), 589–611.

Gentzkow, Matthew and Emir Kamenica, "Bayesian persuasion," *American Economic Review*, 2011, *101* (6), 2590–2615.

Gino, Francesca, Michael I Norton, and Roberto A Weber, "Motivated Bayesians: Feeling Moral While Acting Egoistically," *Journal of Economic Perspectives*, 2016, *30* (3), 189–212.

Gneezy, Uri, Silvia Saccardo, Marta Serra-Garcia, and Roel van Veldhuizen, "Motivated self-deception, identity, and unethical behavior," *unpublished manuscript*, 2015.

Grant, Ruth W, *Strings attached: Untangling the ethics of incentives*, Princeton University Press, 2011.

Greene, William, "The behaviour of the maximum likelihood estimator of limited dependent variable models in the presence of fixed effects," *The Econometrics Journal*, 2004, *7* (1), 98–119.

Hébert, Benjamin and Michael Woodford, "Rational inattention with sequential information sampling," *unpublished*, 2017.

Held, P.J., F. McCormick, A. Ojo, and J.P. Roberts, "A Cost-Benefit Analysis of Government Compensation of Kidney Donors," *American Journal of Transplantation*, 2016, *16* (3), 877–885.

Holt, Charles A. and Susan K. Laury, "Risk Aversion and Incentive Effects," *American Economic Review*, 2002, *92(5)*, 1644 – 1655.

Horton, John J., David G. Rand, and Richard J. Zeckhauser, "The online laboratory: Conducting experiments in a real labor market," *Experimental Economics*, 2011, *14*, 399–425.

Hossain, Tanjim and Ryo Okui, "The binarized scoring rule," *The Review of Economic Studies*, 2013, *80* (3), 984–1001.

Hughes, Jonathan, David Hunter, Mark Sheehan, Stephen Wilkinson, and Anthony Wrigley, *European textbook on ethics in research*, Publications Office of the European Union, 2010.
Informed Consent

***Informed Consent**, Stanford Encyclopedia of Philosophy, 2017.*

Kahneman, Daniel, Jack L Knetsch, and Richard Thaler, *"Fairness as a constraint on profit seeking: Entitlements in the market," The American Economic Review, 1986, 76 (4), 728–741.*

Kamenica, Emir, *"Contextual inference in markets: On the informational content of product lines," The American Economic Review, 2008, 98 (5), 2127–2149.*

_ , *"Behavioral economics and psychology of incentives," Annual Review of Economics, 2012, 4 (1), 427–452.*

Kanbur, Ravi, *"On obnoxious markets," in Stephen Cullenberg and Prasanta Pattanaik, eds., Globalization, Culture and the Limits of the Market: Essays in Economics and Philosophy, Oxford University Press, 2004.*

Klayman, Joshua, *"Varieties of confirmation bias," Psychology of learning and motivation, 1995, 32, 385–418.*

Koszegi, Botond, *"Ego Utility, Overconfidence, and Task Choice," Journal of the European Economic Assiciation, 2006, 4 (4), 673–707.*

Kunda, Ziva, *"The case for motivated reasoning.," Psychological bulletin, 1990, 108 (3), 480.*

Leider, Stephen and Alvin E Roth, *"Kidneys for sale: Who disapproves, and why?," American Journal of Transplantation, 2010, 10 (5), 1221–1227.*

Linnainmaa, Juhani T, Brian T Melzer, and Alessandro Previtero, *"The Misguided Beliefs of Financial Advisors," Unpublished manuscript, 2016.*

livingdonor101.com, *"Living Donor Informed Consent,"* http://www.livingdonor101.com/informedconsent.shtml *November 30 2013.*

Macklin, Ruth, *"'Due' and 'Undue' Inducements: On Paying Money to Research Subjects," IRB, 1981, 3 (5), 1–6.*

Maniadis, Zacharias, Fabio Tufano, and John A List, *"One swallow doesn't make a summer: New evidence on anchoring effects," The American Economic Review, 2014, 104 (1), 277–290.*

Mason, Winter and Siddarth Suri, *"Conducting behavioral research on Amazon's Mechanical Turk," Behavioral Research, 2012, 44, 1–23.*

Matějka, Filip and Alisdair McKay, *"Rational inattention to discrete choices: A new foundation for the multinomial logit model," American Economic Review, 2015, 105 (1), 272–98.*

McGregor, Joan, *"'Undue Inducement'as Coercive Offers'," The American Journal of Bioethics, 2005, 5 (5), 24–25.*

Mitchell, Matthew F and Andrea Moro, *"Persistent distortionary policies with asymmetric information," The American Economic Review, 2006, 96 (1), 387–393.*

Morgan, MA, Jason Dana, George Loewenstein, S Zinberg, and J Schulkin, *"Interactions of doctors with the pharmaceutical industry," Journal of Medical Ethics, 2006, 32 (10), 559–563.*

Morris, Stephen and Philipp Strack, *"The Wald Problem and the Equivalence of Sequential Sampling and Static Information Costs," unpublished, 2017.*

National Bioethics Advisory Commission, *"Ethical and policy issues in research involving human participants," Rockville, MD 2001.*

National Collegiate Athletic Association, *"Division 1 Manual," Indianapolis, IN July 2015.*

Niederle, Muriel and Alvin E Roth, *"Philanthropically Funded Heroism Awards for Kidney Donors," Law & Contemporary Problems, 2014, 77, 131–144.*

Nussbaum, Martha C, *"Objectification," Philosophy & Public Affairs, 1995, 24 (4), 249–291.*

Peterson, Cameron R. and Lee Roy Beach, *"Man As An Intuitive Statistician," Psychological Bulletin, 1967, 68 (1), 29–46.*

Rabin, Matthew and Joel L Schrag, *"First impressions matter: A model of confirmatory bias," Quarterly Journal of Economics, 1999, pp. 37–82.*

Raven, John C, *Guide to the standard progressive matrices: sets A, B, C, D and E, Pearson, 1960.*

Ripley, Elizabeth BD, *"A review of paying research participants: It's time to move beyond the ethical debate," Journal of Empirical Research on Human Research Ethics, 2006, 1 (4), 9–19.*

Rosenberg, Tina, *"It's Time to Compensate Kidney Donors," The New York Times August 7 2015.*

_ , *"Need a Kidney? Not Iranian? You'll Wait.," The New York Times July 31 2015.*

Roth, Alvin E, *"Repugnance as a Constraint on Markets," Journal of Economic Perspectives, 2007, 21 (3), 37–58.*

Sandel, Michael J, *What money can't buy: the moral limits of markets, Macmillan, 2012.*

Satz, Debra, *Why Some Things Should Not Be For Sale: The Moral Limits of Markets, Oxford University Press, 2010.*

Sen, Amartya, *"Well-being, agency and freedom: the Dewey lectures 1984," The journal of philosophy, 1985, 82 (4), 169–221.*

Sen, Amaryta, *On ethics and economics, Oxford University Press, 1999.*

Sims, Christopher A, *"Implications of rational inattention," Journal of Monetary Economics, 2003, 50 (3), 665–690.*

_ , *"Rational inattention: Beyond the linear-quadratic case," American Economic Review, Papers & Proceedings, 2006, 96 (2), 158–163.*

Smith, Vernon L, *"Experimental economics: Induced value theory," The American Economic Review, 1976, pp. 274–279.*

Suen, Wing, *"The Self-Perpetuation of Biased Beliefs," The Economic Journal, 2004, 114 (495), 377–396.*

Svitnev, Konstantin, *"Legal regulation of assisted reproduction treatment in Russia," Reproductive Biomedicine Online, 2010, 20 (7), 892–894.*

Tellioglu, G, I Berber, I Yatkin, B Yigit, T Ozgezer, S Gulle, G Isitmangil, M Caliskan, and I Titiz, *"Quality of life analysis of renal donors," Transplantation proceedings, 2008, 40 (1), 50–52.*

Thiessen, C, YA Kim, R Formica, M Bia, and S Kulkarni, *"Written informed consent for living kidney donors: practices and compliance with CMS and OPTN requirements," American Journal of Transplantation, 2013, 13 (10), 2713–2721.*

Titmuss, Richard, *The gift relationship: From human blood to social policy, London: Allen & Unwin, 1970.*

Toplak, Maggie E, Richard F West, and Keith E Stanovich, *"Assessing miserly information processing: An expansion of the Cognitive Reflection Test," Thinking & Reasoning, 2014, 20 (2), 147–168.*

Tversky, Amos and Daniel Kahneman, *"Judgment under uncertainty: Heuristics and biases," Science, 1974, 185 (4157), 1124–1131.*

US Department of Health, Education, and Welfare (DHEW), *The Belmont report: Ethical principles and guidelines for the protection of human subjects of research, National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, Washington DC, 1978.*

Vatican Radio, *"Pope Francis meets a group of transplant surgeons; including the mayor of Rome,"* http://en.radiovaticana.va/news/2014/09/20/pope_francis_meets_a_group_of_transplant_surgeons/1106931 *September 20 2014.*

Wald, Abraham, *Sequential Analysis, New York: Wiley, 1947.*

Wazana, Ashley, *"Physicians and the pharmaceutical industry: is a gift ever just a gift?," Journal of the American Medical Association, 2000, 283 (3), 373–380.*

ONLINE-APPENDIX

An Offer You Can't Refuse

Incentives Change How We Inform Ourselves

and What We Believe

Sandro Ambuehl

Table of Contents

A Experiment 1: Additional Analysis

A.1 Randomization check

The four treatments are balanced across demographic characteristics. Table A.5 displays summary statistics of these variables by treatment. For each variable, the table reports the p-value of an F-test for differences in the mean value of the variable across treatments. Of 24 tests conducted, one is significant at the 5% level, and an additional three are significant at the 10% level. This is within the expected range.

A.2 Choice consistency

A participants choices are inconsistent if she rejects a transaction at price p in the multiple price list (MPL) in step 3 of the experiment, but accepts the same transaction in step 4, or vice versa.[1] Table A.6 details the fraction of each of these types of inconsistencies by treatment. It shows that subjects in the low incentive treatments tend to state reservation prices that are too high relative to their behavior in their \$3-treatment decision. No such directional bias is evident for subjects in the high incentive condition. This does *not* point to a difference across treatments, as the decisions that reveal inconsistencies differ across the incentive treatments.

The fraction of inconsistent decisions is somewhat higher than is usually found in the literature on decision making under explicit risk, in which inconsistencies are identified by means of multiple switching in a price list (e.g. Holt and Laury 2002). This may be because the decisions that reveal inconsistencies in this experiment are temporally separated, whereas in the risky decision making literature they are typically presented simultaneously.

Condition	*video*	*no video*
Incentive \$3		
Reservation price > \$3 in MPL, accept \$3 in treatment decision	15.03%	16.32%
Reservation price < \$3 in MPL, reject \$3 in treatment decision	1.42%	3.82%
Total	16.45%	20.15%
Incentive \$30		
Reservation price > \$30 in MPL, accept \$30 in treatment decision	4.24%	8.00%
Reservation price < \$30 in MPL, reject \$30 in treatment decision	5.71%	6.37%
Total	9.95%	14.37%

Table A.6: Choice inconsistencies across steps 3 and 4 of the experiment.

[1] The choices subjects' made in step 6 of the experiment cannot reveal any inconsistencies, as they are made with different information about the transaction than the treatment decisions in step 4.

Treatment condition					
Incentive	\$30	\$3	\$30	\$3	
Video	Yes	Yes	No	No	
Variable	Mean				p-value
Male	0.55	0.53	0.54	0.54	1.00
Age	21.43	22.01	21.37	21.30	0.34
Ethnicity					
African-American	0.05	0.06	0.07	0.07	0.70
Caucasian	0.57	0.51	0.59	0.56	0.28
East Asian	0.19	0.26	0.19	0.23	0.22
Hispanic	0.07	0.08	0.04	0.04	0.99
Indian	0.03	0.04	0.04	0.07	0.56
Other	0.08	0.05	0.07	0.04	0.70
Monthly spending in USD	251.72	301.40	289.07	288.42	0.44
Year of study[a]	3.50	3.60	3.61	3.47	0.32
Graduate student	0.13	0.15	0.13	0.05	0.07
Field of study					
Arts and humanities	0.16	0.09	0.13	0.11	0.04
Business or economics	0.27	0.36	0.34	0.43	0.09
Engineering	0.20	0.16	0.11	0.12	0.49
Science	0.21	0.23	0.27	0.23	0.47
Social science (excluding business and economics)	0.17	0.17	0.15	0.11	0.59
Political orientation[b]	0.50	0.32	0.27	0.09	0.08
Raven's score[c]	14.77	14.76	14.69	14.68	1.00
CRT score[d]	3.76	3.80	3.50	3.22	0.08
Experience with insects as food					
Has intentionally eaten insects before	0.19	0.22	0.19	0.20	0.71
Grown up in culture that practices entomophagy	1.30	1.27	1.25	1.31	0.92
Grown up eating mostly western foods	0.81	0.73	0.82	0.78	0.06
Had a pet that fed on store-bought insects	0.25	0.25	0.21	0.26	0.68
Knew that this study concerns insect eating	2.64	2.46	2.54	2.49	0.31

Table A.5: Summary statistics and randomization check. The last column displays the p-value of the test of joint significance of a regression of the indicated variable on treatment dummies.

[a]Year of study only includes undergraduate students.
[b]Political orientation is measured on a scale of -2 (conservative) to 2 (liberal).
[c]Raven's score is measured on a scale of 0 to 24.
[d]CRT score refers to performance on the extended version of the test (Toplak et al., 2014) and is measured on a scale of 0 to 6.

A.3 Estimating reservation prices

In section 3.2, I report the estimates of the second stage of a double hurdle model. Here, I describe the estimating equation, present the full set of estimated coefficients, and test the restriction that the coefficients in the selection and amount equations are equal (as the Tobit model imposes).

Double hurdle model There are n subjects, each of whom provide measures of reservation prices for T species each. I use r_{it} to denote individual i's reservation price for species t and set $y_{it} = 60 - r_{it}$ ($60 is the highest price offered in any decision in this experiment). The two hurdles are defined as follows:

1. Participation decision (first hurdle)

$$\begin{aligned} d_i^* &= z_i'\alpha + \epsilon_{1,i} \\ d_i &= \begin{cases} 1 & \text{if } d_i^* > 0 \\ 0 & \text{otherwise} \end{cases} \end{aligned}$$

2. Amount decision (second hurdle)

$$\begin{aligned} y_{it}^{**} &= x_{it}'\beta + u_i + \epsilon_{2,it} \\ y_{it}^* &= \max(y_{it}^{**}, 0) \end{aligned}$$

The vector of error terms $(\epsilon_{1,i}, u_i, \epsilon_{2,i})$ is normally distributed with mean zero, and variance-covariance matrix

$$\Sigma = \begin{pmatrix} 1 & \rho\sigma_u & 0 \\ \rho\sigma_u & \sigma_u^2 & 0 \\ 0 & 0 & \sigma_\epsilon^2 \end{pmatrix}$$

The observed variable is $y_{it} = d_i y_{it}^*$. The second hurdle contains a subject-specific random-effect term u_i that allows between-subject heterogeneity.

Implementation I estimate this model using the user-written Stata command `xtdhreg` by Engel and Moffatt (2014). I estimate two specifications. In the first, I include demographic controls (gender, ethnicity, age, age^2) as well as university and species dummies in the amount equation. I also allow for correlation in the error terms across the hurdles (that is, ρ is endogenous).

The results from this specification are reported in the main text. In the second, I do not include any controls, and in the third I include only the demographic controls. I also demonstrate that a double hurdle model is the appropriate specification. Do do so, for each of the two specifications I also estimate a version in which I impose the restriction the Tobit model makes, namely that the treatments affect the participation decision and the amount decision in the same way. Formally, I require $\alpha_k = \beta_k$ for all variables k that are included in both x_i and z_i. For each of the specifications, I use a likelihood ratio test to test that restriction. Finally, I also estimate a specification including further controls (see section A.7 for details).

Results Table A.7 displays the results. A comparison of Columns 1 and 3 shows that the estimates are not significantly affected by the inclusion of demographic control variables, and one of Columns 1 and 5 shows that the qualitative results are not substantially affected by the inclusion of university and species fixed effects. Comparing Columns 1 and 7 further reveals that the adding further control variables to the amount equation does not substantially change any qualitative results either.

For the first, second, and third specification, the value of the likelihood ratio test statistic is 14.54, 12.88 and 11.74, respectively. Each exceeds the 5%-critical value of the $\chi^2(4)$ distribution of 9.49. Hence, the restricted versions are rejected in favor of the unrestricted ones. The interpretation is that subjects who do not consume any insect for any price in this experiment are differently affected by the treatment interventions than the remaining subjects, as one would expect given the treatment incentives of \$3 and \$30 are less than half of those subjects' reservation prices.

A.4 Awareness

I obtain the results on subjects' awareness of self-persuasion discussed in Section 3.3 by estimating the following regression model, separately for subjects in the *video* condition and for subjects in the *no video* condition. (Recall that subjects in the *video* (*no video*) condition predicted the reservation price of other subjects in the *video* (*no video*) only.)

$$\widehat{WTA}^{i}_{cs} = \beta_0 + \beta_1 \mathbb{1}(\text{incentive}_i = \text{high}) + \beta_2 \mathbb{1}(c = \text{high}) + \epsilon^{i}_{cs} \tag{3}$$

Here, $\widehat{WTA}^{i}_{cs}$ is subject i's prediction of the the mean reservation price of subjects in incentive condition c for species s. In words, I regress subject i's prediction about others' reservation price on a dummy that indicates whether the prediction concerns a previous subject facing $c = \$3$ or $c = \$30$ low incentives. Hence, β_2 is the amount by which subjects' predict incentives change reservation prices for others. I let the intercept vary depending on whether the subject making the prediction was herself offered the high or the low incentives.

Variable	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	Reservation price elicited in stage 3													
Restriction	Unrestricted		Restricted		Unrestricted		Restricted		Unrestricted		Restricted		Unrestricted	
Equation	Particip.	Amount	Particip.	Amount	Particip.	Amount	Particip.	Amount	Particip.	Amount	Particip.	Amount	Particip.	Amount
High incentive	-0.04	5.97***	-0.06	-0.06	-0.02	6.29***	-0.04	-0.04	-0.03	6.39***	0.06	-0.06	-0.03	6.04**
	(0.19)	(2.26)	(0.19)	(0.19)	(0.19)	(2.41)	(0.19)	(0.19)	(0.19)	(2.26)	(0.19)	(0.19)	(0.19)	(2.40)
Video	-0.05	-0.07	-0.05	-0.05	-0.03	1.13	-0.03	-0.03	-0.04	0.55	0.05	-0.05	-0.07	-1.67
	(0.18)	(2.32)	(0.19)	(0.19)	(0.18)	(2.17)	(0.18)	(0.18)	(0.18)	(2.18)	(0.19)	(0.19)	(0.18)	(2.35)
High inc. × Video	0.11	-7.25**	0.13	0.13	0.05	-9.09***	0.09	0.09	0.08	-9.05***	-0.14	0.14	0.10	-6.67**
	(0.26)	(3.06)	(0.26)	(0.26)	(0.26)	(3.25)	(0.26)	(0.26)	(0.26)	(3.09)	(0.26)	(0.26)	(0.26)	(3.20)
Constant	-1.00***	-	-1.00***	-	-1.00***	39.53***	-1.00***	38.09***	-1.01***	-	-1.00***	-	-1.00***	-
	(0.13)		(0.13)		(0.13)	(1.61)	(0.13)	(0.93)	(0.13)		(0.14)		(0.14)	
Controls														
Demographic	No	Yes	No	Yes	No	No	No	No	No	Yes	No	Yes	No	Yes
Other	No	No	No	No	No	No	No	No	No	No	No	No	No	Yes
University dummies	No	Yes	No	Yes	No	No	No	No	No	No	No	No	No	Yes
Species dummies	No	Yes	No	Yes	No	No	No	No	No	No	No	No	No	Yes
σ_u	19.33		19.57		19.69		19.81		19.14		19.66		18.66	
σ_ϵ	9.97		9.98		10.66		10.67		10.67		10.67		9.97	
ρ	0.46		0.45		0.43		0.40		0.46		0.47		0.48	
Log likelihood	-9512.23		-9519.58		-9663.75		-9670.19		-9644.43		-9650.30		-9492.52	
Observations	3,276		3,276		3,276		3,276		3,276		3,276		3,276	
Subjects	671		671		671		671		671		671		671	

Table A.7: Estimation results of the double hurdle model specifications. Each estimation result consists of two equations labeled *particip.* and *amount*, corresponding to the parameters in the participation and amount equations, respectively. Whenever control variables are included, the constant is not easily interpretable, and thus not reported. The model in each even numbered Column is the same as the one immediately to the right, with the restriction that the coefficients of the treatment variables are the same across the two equations. The model in Column 7 parallels that in Column 1 but includes an additional set of control variables described in Section A.7. The number of observations falls short of $671 \times 5 = 3355$ by 79 since the first 79 subjects did not make any decisions about field crickets.

I compare subjects' predictions of the effect of incentives on reservation prices to their actual effect. To simplify the comparison, I estimate the following linear model.

$$WTA_s^i = \gamma_0 + \gamma_1 \mathbb{1}(\text{incentive}_i = \text{high}) + \eta_s^i \tag{4}$$

I jointly estimate equations (3) and (4), jointly across both video conditions. I control for gender, ethnicity, age and age^2, and include university and species fixed effects.

Column 1 of table A.8 displays the estimated parameters of equation (3) for subjects in the *no video* condition. It shows that these subjects predicted that other subjects in the *no video* condition would demand an additional \$4.07 to eat an insect when offered the high rather than the low incentive. Column 2 displays the estimate of the effect of incentives on actual reservation prices, and shows an effect of \$4.45. Subjects' predictions deviate from the measured effect of \$4.45 by a statistically and economically insignificant \$0.37.

Columns 3 and 4 show the respective data for the *video* condition. Subjects predict that the effect of incentives on other subjects in the *video* condition is \$5.16, and thus predict the anchoring effect of \$4.45 (as measured in the *no video* condition) with reasonable accuracy. In reality, however, that effect is countervailed by a sizable self-persuasion effect. These two effects sum to a *negative* \$0.55. Hence, the predictions of subjects in the *video* treatment are wildly off, by a highly significant \$5.70. On average, subjects lack awareness of the self-persuasion effect.

Even though subjects do not predict the self-persuasion effect, their predictions are affected by it. Apparently, subjects who have both an incentive and the opportunity to persuade themselves do so. But because they lack awareness of this effect, they project their own lower willingness to accept onto others. Column 3 of Table A.8 shows that amongst subjects in the *video* condition, those who were given the high incentive predict significantly lower reservation prices for others. For those who could not see a video, this effect is just 40% as large, and not statistically significant.

Subjects' beliefs about the effect of incentives on others are heterogenous. Roughly one third of subjects predict that higher incentives decrease reservation prices, and roughly two-thirds predict the opposite. These fractions does not substantively differ across the four treatment cells ($p = 0.79$, test for joint significance of treatment dummies). Hence, while the above results show that subjects on average fail to predict the self-persuasion effect, it is still possible, for instance, that subjects who are themselves more prone to self-persuasion are more likely to predict a negative effect of incentives on reservation prices. To address this issue, I split the sample into those who predict that incentives lower reservation prices, and those who predict the opposite. I can do so because each subject separately predicted the reservation prices of

	(1)	(2)	(3)	(4)
	no video		*video*	
Dependent variable	Reservation price		Reservation price	
	predicted	actual	predicted	actual
Level with \$3 incentive	20.35	22.31	18.76	20.63
	(2.16)	(2.22)	(1.22)	(1.37)
Effect of increase in incentive	4.07***	4.53*	5.16***	- 0.63
	(0.72)	(2.55)	(0.63)	(2.05)
Effect of predictor's own incentive on prediction	-0.95	-	-2.72**	-
	(1.60)		(1.34)	
Difference predicted vs. actual effect of increase in incentive	-0.46	-	5.79***	-
	(2.65)		(2.03)	
Observations	2,710	1,355	3,520	1,921
Number of subjects	271	127	352	195

Table A.8: Demographic controls are gender, ethnicity, age and age^2. Levels are displayed for the mean participant. The first 48 participants at Stanford did not predict others' reservation prices, and are therefore not included in the regression in this table. Estimated using university and species fixed effects. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

previous participants in the high and low incentive conditions. These predictions cannot be an ex-post rationalization of subjects' own behavior, since each subject was in only one treatment.

Indeed, subjects who expect self-persuasion in others are indeed more prone to self-persuasions themselves. Amongst the 24.25% of subjects in the *video* condition who believe that self-persuasion outweighs anchoring, the increase in incentives indeed leads to a \$5.83 *decrease* in reservation prices. Amongst the 69.5% of subjects in the *video* condition with the opposite beliefs, by contrast, the same increase in incentives leads to a \$2.49 increase in reservation prices. The difference between these effects is significant at the 10%-level. Hence, subjects are *partially* aware of the effect of incentives on reservation prices.[2]

[2] Amongst subjects in the *no video* condition, 31% believe that self-persuasion outweighs anchoring, and 63.1% have the opposite beliefs. The effect of the increase in incentives on those subjects' reservation prices are \$8.39 and \$2.01, respectively. The p-value of the difference is $p = 0.102$.

	Low Incentive	High Incentive	Difference High - Low
Information			
no video	22.10	26.68	4.58*
	(1.82)	(1.81)	(2.55)
video	19.64	16.99	-2.65
	(1.72)	(1.69)	(2.35)
Difference	-2.46	-9.69***	-7.23**
	(2.42)	(2.44)	(3.43)

Table A.9: Reservation prices after distribution of insects, in dollars, estimated by the second equation of the double hurdle model using 3,226 observations from $n = 671$ subjects. Standard errors clustered by subjects. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Asterisks are suppressed for levels.

A.5 Reservation prices after distribution of insects

In step 5 of the experiment, subjects receive all the insects. This potentially changes reservation prices, due to a multitude of factors. On the one hand, the additional information one can gather by observing the insects is still imperfect about what it is like to ingest them, so that skewed interpretation is still possible. On the other hand, the additional information might lead to some convergence of beliefs towards the true disutility from consuming the insects. Moreover, because the decision that will be carried out with the highest likelihood has already been made, *ex post* rationalization may now affect behavior. Whatever the mechanism, table A.9 displays the mean reservation prices after the distribution of the insects as estimated by the second stage of the double hurdle model.[3]

A.6 Choice of video clips

Subjects in the *video* condition also select at least four out of a selection of 9 video clips, grouped in bins of three named "Reasons for eating insects", "Reasons against eating insects", "Other information about eating insects". This reveals whether incentives affect the *amount* of information demanded.

Subjects know that they will either watch the selected 6-minute video, or all the clips they selected, but not both. They also know that the chance of the former is 97%. This probability is chosen for reasons of statistical power. There are many possible selections of video clips, each

[3] I use the same set of control variables as in the main text. Other estimated parameters: $\sigma_u = 19.25$, $\sigma_\epsilon = 10.87$. $\rho = 0$ is enforced, as the likelihood function is flat when this parameter is endogenous.

of which could potentially differently affect behavior. By contrast, there are only two selections of 6-minute videos, thus leading to a potentially more pronounced treatment effect.

Empirically, subjects' choice of video clips reinforces the finding that incentives cause them to demand information in a way that is more favorable to participation. Table A.10 shows that subjects in the $30-condition select significantly fewer discouraging clips, and significantly more encouraging clips, while the number of other clips is unaffected. Incentives do not affect the total number of clips selected, because most subjects opt for the minimum number of four clips.

	(1)	(2)	(3)	(4)
	Number of clips chosen			
	Encouraging	Discouraging	Other	Total
Effect of higher incentive	0.17*	-0.24**	0.01	-0.07
	(0.10)	(0.11)	(0.08)	(0.11)
Levels				
$30 incentive	2.29	0.99	1.15	4.42
	(0.06)	(0.08)	(0.06)	(0.11)
$3 incentive	2.12	1.23	1.14	4.49
	(0.07)	(0.08)	(0.06)	(0.09)
Observations	321	321	321	321

Table A.10: Video clips chosen by incentive condition. The number of observations is smaller for the video clips than for the six minute video as the first 79 participants at Stanford could not choose any clips. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Asterisks are suppressed for levels.

A.7 Robustness checks

Video choice and participation probabilities Table A.12 reports estimates of alternative regression specifications of the results on information choice and participation probabilities. First, I estimate all results with a wider range of control variables. In addition to gender, ethnicity, and age, I also control for monthly spending, score in the CRT test, score in the IQ test, college major, as well as variables relating to experience with insects. The latter are whether the subject has voluntarily eaten insects before, whether the subject has grown up eating mostly western foods, and whether the subject has a background in a culture in which entomophagy is common. Second, I estimate Probit and Logit specifications. (For these regressions, I do not include university and species fixed effects, see Greene 2004.)

	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Choose encouraging video					
Effect of high incentive	0.07*	0.07**	0.32**	0.35**	0.56*	0.58*
	(0.04)	(0.03)	(0.16)	(0.16)	(0.29)	(0.30)
Method	OLS	OLS	Probit	Probit	Logit	Logit
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes
University FE	Yes	Yes	No	No	No	No
Species FE	Yes	Yes	No	No	No	No
Observations	2,000	2,000	1,920	1,920	1,920	1,920
Subjects	400	400	384	384	384	384

Table A.11: Robustness checks for treatment effects on the probability of choosing the encouraging rather than the discouraging video. *Other controls* are monthly spending, score in the CRT test, score in the IQ test, college major, as well as variables relating to experience with insects. The latter are whether the subject has voluntarily eaten insects before, whether the subject has grown up eating mostly western foods, and whether the subject has a background in a culture in which entomophagy is common. The number of observations for the probit and logit models is reduced since for some realizations of control variables, the outcome is predicted perfectly (for instance subjects identifying as neither male nor female). Column 1 replicates the respective specification reported in the main text. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Accept transaction at promised incentive					
Effect in *no video*	0.22***	0.23***	0.60***	0.61***	0.98***	1.02***
	(0.05)	(0.05)	(0.13)	(0.13)	(0.22)	(0.22)
Effect in *video*	0.33***	0.33***	0.91***	0.93***	1.48***	1.55***
	(0.04)	(0.04)	(0.11)	(0.12)	(0.19)	(0.20)
Difference in effects	0.10*	0.10*	0.31*	0.32*	0.51*	0.53*
	(0.06)	(0.06)	(0.18)	(0.18)	(0.29)	(0.29)
Method	OLS	OLS	Probit	Probit	Logit	Logit
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes
University FE	Yes	Yes	No	No	No	No
Species FE	Yes	Yes	No	No	No	No
Observations	3,307	3,307	3,288	3,288	3,288	3,288
Subjects	671	671	667	667	667	667

Table A.12: Robustness checks for treatment effects on the supply curves. See Table A.12 for explanation of variables. Column 1 replicates the respective specification reported in the main text. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

Reservation prices The estimates of reservation prices with the additional control variables described in the previous paragraph are displayed in Column 5 of Table A.7. None of the results are relevantly different.

In addition to the double hurdle specifications, I also use linear specifications, both with and without additional statistical control variables, and interval regression. I estimate each of these specifications twice. The first time, I only include the 90.61% *complier* subjects. These are the subjects who accept at least one out of the 10 offers in the experiment to eat an insect in exchange for \$60. The second time, I also include the remaining 9.39% of subjects who categorically refuse to eat insects, even for an amount that is twice the 'high' incentive of \$30. These subjects cannot reasonably be expected to be affected by the incentive treatment. Because of their extreme reservation prices (set to \$60 in the linear specifications), however, their presence substantially lowers the statistical precision of the estimates.

Table A.13 reports the results. Considering first the results on the *complier* subjects alone, these specifications replicate the significant interaction between the incentive and video conditions, as in the main text. The estimated coefficient is attenuated in the linear specifications that ignore the censoring issue (Columns 1 and 3). The inclusion of additional control variables only marginally alters the estimated coefficients. Considering the results on the sample including all subjects we find coefficients that are slightly attenuated, and standard errors that are slightly larger, as one would expect from adding what essentially amounts to noise to the data. While the estimated coefficients on the interaction between the incentive and video condition loses statistical significance, they remain largely similar in magnitude to the case in which only *complier* subjects are included, across all three specifications.

	(1)	(2)	(3)	(4)	(5)	(6)
Variable			Reservation price			
Method	OLS	OLS	OLS	OLS	iReg	iReg
Sample	Compliers	All	Compliers	All	Compliers	All
High incentive × video	-6.55**	-5.15	-5.94**	-4.89	-7.36*	-5.33
	(3.11)	(3.22)	(3.00)	(3.13)	(3.79)	(4.38)
High incentive	5.30**	4.56*	5.04**	4.52*	5.90**	4.68
	(2.42)	(2.49)	(2.36)	(2.44)	(2.94)	(3.42)
Video	1.05	-0.65	0.88	-0.59	1.14	-1.19
	(2.16)	(2.27)	(2.15)	(2.24)	(2.65)	(3.09)
Constant	11.09	17.27	14.66	19.31	9.16	15.81
	(11.63)	(12.60)	(11.65)	(12.69)	(14.14)	(16.67)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	No	No	Yes	Yes	No	No
University dummies	Yes	Yes	Yes	Yes	Yes	Yes
Species dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,961	3,276	2,961	3,276	2,961	3,276
Subjects	608	671	608	671	608	671

Table A.13: Robustness checks for treatment effects on reservation prices. See Table A.12 for explanation of variables. iReg refers to interval regression. Standard errors clustered by subject. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

A.8 Subjects who watched the encouraging video

Here I replicate Table 3 comparing subjects in the *video* condition who watched the encouraging video to subjects in the *no video* treatment. This provides supporting evidence for the finding that incentives affect participation probabilities and reservation prices not only though their effect on video choice, but also by changing how subjects interpret the information in the encouraging video. These estimates need to be interpreted with caution, as they are subject to endogeneity and selection bias because the video a subject watched reflects a choice. The estimated coefficients remain largely comparable to those I obtain when all subjects are included.

	A. Supply curves			B. Reservation prices		
Incentive	\$3	\$30	*Difference*	\$3	\$30	*Difference*
Information						
no video	37.08	59.43	22.34***	20.11	24.90	4.79**
	(3.39)	(3.39)	(4.77)	(1.72)	(1.86)	(2.31)
video	40.94	74.38	33.44***	17.64	17.58	-0.05
	(3.41)	(2.53)	(4.21)	(1.73)	(1.45)	(2.18)
Difference	3.86	14.95**	11.10*	-2.47	-7.32***	- 4.85
	(4.83)	(4.31)	(6.37)	(2.25)	(2.25)	(3.23)

Table A.14: Replication of Table 3 including only subjects who either had chosen to watch the encouraging video, or were in the control treatment. Standard errors clustered by subjects. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Asterisks are suppressed for levels.

B Model: Additional Materials and Proofs

B.1 General state space

In this section, I show that the assumption of two states of the world made in section 4 is inessential to the qualitative predictions of the model. I extend the model to a general state space $\Omega \subseteq \mathbb{R}$. This allows a decision maker to learn not only about the likelihood that the consequence of a transaction will be good or bad, but also the distribution of the magnitudes of the gains and losses that can be incurred.

I still find that higher incentives increase the demand for information about the upside of the transaction, and decrease the demand for information about the downside. I also find that if the costs of information are proportional to Shannon mutual information, then an increase in incentives increases the probability an agent *ex post* regrets participating *conditional on having participated*. In addition, I show that posterior-separability of the cost of information function (in the sense of Caplin and Dean (2013b)) is sufficient for higher incentives to increase the false positive rate.

Setup An agent whose preferences are quasilinear in money can decide whether or not to participate in a transaction. If he abstains, he receives utility 0. If he accepts, he receives a monetary payment $m \geq 0$, and stochastic, non-monetary utility $u(\omega)$ with $u : \Omega \to \mathbb{R}$ increasing, and $u(0) = 0$, where Ω is a measurable subset of $\mathbb{R}$ that represents the set of states of the world. The agent is imperfectly informed about ω and thus about his utility from accepting the transaction. His prior distribution of ω is given by a probability density function $\mu(\omega)$.

Before deciding whether or not to accept the transaction, the agent can obtain information about ω. As in section 4, I directly model the agent as choosing state-dependent participation probabilities $p_\omega = P(\text{accept}|\omega)$. The cost of a vector of state dependent acceptance probabilities $(p_\omega)_\omega$ is given by $c\big((p_\omega)_\omega\big) \in \mathbb{R}$. Hence, the setting differs from that in section 4 only to the extent that the state space Ω is more general.

Analysis The agent's utility from state dependent acceptance probabilities $(p_\omega)_\omega$ is

$$V = E\left[(\omega + m)p_\omega\right] - c\big((p_\omega)_\omega\big) \tag{5}$$

where the expectation is taken with respect to the agent's prior beliefs. To illustrate, notice that with incentives m, a perfectly informed agent would accept if $\omega + m \geq 0$ and reject otherwise.

I consider an increase in the incentive for participation from m to $m' > m$. In contrast to the two-states model, such a change now does not only affect the costs of false positives and false negatives, but also changes the set of states ω in which the agent would participate under full information. Nonetheless, an increase in the incentive still leads to the substitution and stakes effects outlined in section 4. On the one hand, higher incentives change the stakes of the decision, and thus lead the agent to acquire a different amount of information. If it causes the agent to purchase more information, he will increase p_ω for those ω in which accepting is optimal, and decrease p_ω for the ω for which rejection is optimal. On the other hand, higher incentives make false positives cheaper, and they make false negatives more expensive. Hence, the agent will acquire a different kind of information; p_ω will increase for all ω, including those for which rejection is optimal. Which of these effects outweighs depends on the cost of information function. Figure B.7 illustrates.

As in the two-state model, *posterior separability* (Caplin and Dean, 2013b) of the cost of information function is a sufficient condition for the substitution effect to outweigh (higher incentives increase false positives). In the continuous case, it is given as follows. I write $p = E(p_\omega) = \int p_\omega \mu_\omega d\omega$ for the unconditional probability that the agent participates if his state-contingent participation probabilities are given by $(p_\omega)_\omega$. Moreover, I write $\gamma_a^\omega = \frac{p_\omega \mu}{p}$ for the value of the posterior density at ω if the agent participates, and $\gamma_r^\omega = \frac{(1-p_\omega)\mu}{1-p}$ if he abstains. The cost function c is *posterior separable* if there exists a strictly convex function $h : [0,1] \to \mathbb{R}$ such that c can be written in the following form[4]

$$c\big((p_\omega)_\omega\big) = E\left[h(\mu)\right] - pE\left[h(\gamma_a^\omega)\right] - (1-p)E\left[h(\gamma_r^\omega)\right]$$

[4]This is the Shannon mutual information cost function if $h(x) = x\log(x) + (1-x)\log(1-x)$.

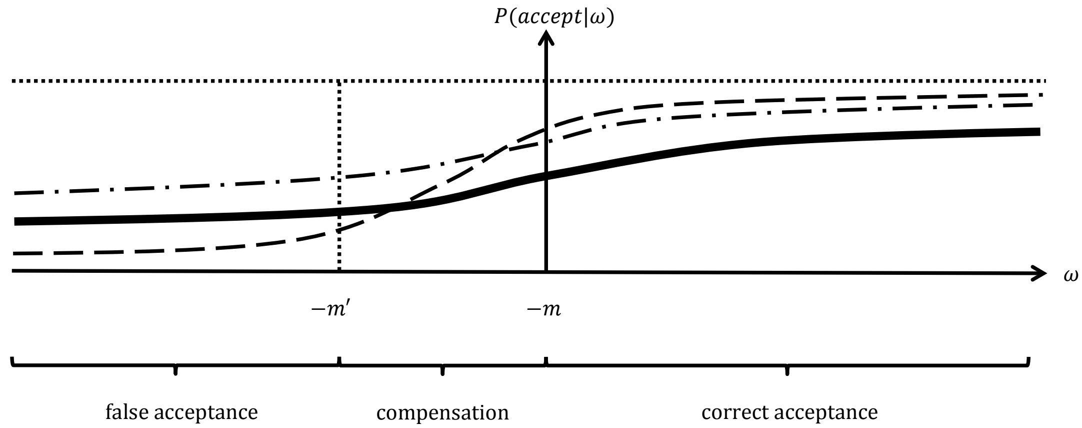


Figure B.7: Effects of a change in incentives on the agents' information with $u(\omega) = \omega$. The figure plots, for each state of the world ω the probability that the agent accepts the offer given his optimal information demand. With incentives $m = 0$, optimal information demand could, for instance, lead to a schedule of acceptance probabilities $P(accept|\omega)$ depicted by the bold line. An increase in incentives to $m' > m$ increase $P(accept|\omega)$ for all $\omega > -m$, if the income and stakes effects have the same direction. Depending on the cost of information function, the stakes effect or the substitution effect may dominate for $\omega < -m'$. These cases are illustrated by the dashed and dot-dashed schedules, respectively. In case of a posterior-separable cost of information function, the substitution effect dominates.

Part (i) of the following proposition formally shows that an increase in incentives increases the false positive rate if the cost function is posterior separable. Specifically, for all $\omega < -m'$, the agent will ex-post regret if he participates if the state is ω. Because under posterior separability all p_ω increase, this means that in particular the false positive probability increases. Note also that posterior separability is also a sufficient condition for the required sign of the cross-derivative in the special version of this model outlined in section 4.

To generalize proposition 2, note in the two-state model participation at a lower posterior implies an increased probability that the agent *ex post* regrets participation *conditional on having participated*. Part (ii) of the proposition below shows that this statement generalizes to the continuous state if c is given by Shannon mutual information costs, and if the change in the incentive amount has a sufficiently small effect on the measure of states in which a fully informed agent would change his participation decision.

Proposition 3.

(i) If c is posterior separable, and if $m' > m$, then for all $\omega \in \Omega$, $p_\omega(m') \geq p_\omega(m)$.

(ii) If c is proportional to Shannon mutual information, and $P\big(\omega \in [-m', m]\big)$ is sufficiently small, then an increase in incentives from m to $m' > m$ increases the probability the agent regrets conditional on having participated.

B.2 Predictions for the experiment in section 5

The experiment in section 5 maps directly to the model in section 4. Here, I formally derive the predictions I test with that experiment.

Slope of the supply curve Consider incentive amounts $\bar{m}$ and $\underline{m}$ with $-\pi_B > \bar{m} > \underline{m} > 0$. Consider two treatments. In the *incentive first* treatment, the agent first learns the incentive $m \in \{\bar{m}, \underline{m}\}$, then gathers information, and finally decides whether to take the bet. In the *picture first* treatment, the agent first only learns that the incentive will be $\bar{m}$ or $\underline{m}$ with probability α and $(1-\alpha)$, respectively. He then gathers information, then learns the realization of m, and finally decides whether participate. I say that information is instrumental if the agents' betting decision depends on the realization of the informative signal.

Proposition 4. *Suppose the cost-of-information function c has increasing differences. Let $p_b(m)$ and $p_a(m)$ denote the* ex ante *participation probabilities in the two treatments if the incentive is m. Suppose the chosen information is instrumental for both incentive amounts in both the* incentive first *and the* picture first *condition. Then, $p_b(\bar{m}) > p_a(\bar{m})$ and $p_b(\underline{m}) < p_a(\underline{m})$. In particular, the effect of incentives is stronger in the* incentive first *condition: $p_b(\bar{m}) - p_b(\underline{m}) > p_a(\bar{m}) - p_a(\underline{m})$.*

The proof is in section B.3. Intuitively, the result can easily be seen in Figure B.8. The subjects' objective function in the *picture first* condition regarding information acquisition is simply $\alpha U(p_G, p_B; \bar{m}) + (1-\alpha) U(p_G, p_B; \underline{m}) = \mu p_G(\pi_G + \hat{m}) + (1-\mu) p_B(\pi_B + \hat{m})$, where $\hat{m} = \bar{m} + \underline{m}$. Hence, the decision maker chooses the same information structure he would choose if the incentive amount were equal to the expected incentive. This leads to a choice of false positive and false negative probabilities that are between those he would choose if he knew the realization of the incentive amount before acquiring information.

The condition of this proposition is that information is instrumental for both incentive amounts in both information conditions. To see that it is satisfied empirically, compare panels B and C of Table 4. For both incentive amounts in both information conditions, the participation probability is higher by a factor of about 2 or more if the state is good than if it is is bad. Hence, the information about the state does affect subjects' participation probability.

If information acquisition cannot depend on the incentive, and the subject follows the signal, this model predicts that the increase in the incentive has no effect at all. A variety of extensions allow incentives to affect participation in the *picture first* condition even if information is instrumental. The most plausible one is that a fraction of subjects remember, perhaps vaguely, the picture they have seen. Once they learn which incentive amount has realized, they mentally revisit the picture, and decide whether to take the lottery. In this case, the *picture first* condition does not entirely preclude incentive-dependent information acquisition. (A related possibility is that some subjects take a screenshot, and further examine the picture once they know what incentive has realized.)

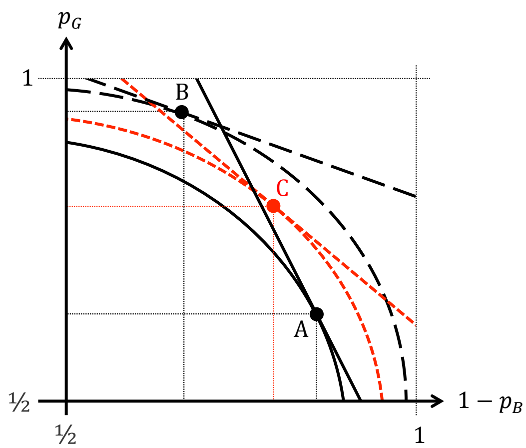


Figure B.8: Effects of having to choose an information structure before the incentive amount is known. As in figure 3, points A and B represent optimal information demand with low ($\underline{m}$) and high ($\bar{m}$) incentives, respectively. Point C represents the optimal information demand if information is chosen knowing solely that the incentive amount will be $\underline{m}$ or $\bar{m}$, each with equal probability.

B.3 Proofs

Proof of proposition 1

(i) For convenience, let $q_G = p_B$, $q_B = 1 - p_B$, and let $q = (q_G, q_B)$. Without loss of generality, we set $m = 0$ (π_G and π_B can be adjusted accordingly) and $\lambda = 1$. The agent solves

$$\max_q \mu q_G \pi_G + (1-\mu)(1-q_B)\pi_B - c(q)$$

We derive the comparative statics regarding the payoffs π_G, and π_B. By the limit assumptions on c, whenever the optimal solution involves a positive amount of information acquisition, the optimal participation probabilities are interior. Since the cost function is strictly convex, any interior solution q^* to the maximization problem is characterized by the first order condition

$$\begin{bmatrix} \mu & 0 \\ 0 & -(1-\mu) \end{bmatrix} \begin{bmatrix} \pi_G \\ \pi_B \end{bmatrix} = \nabla c(q^*)$$

Let $D^2 c = \left[\begin{smallmatrix} c_{11} & c_{12} \\ c_{21} & c_{22} \end{smallmatrix}\right]$ denote the Hessian of the cost function. Then, totally differentiating the foregoing condition yields

$$\begin{bmatrix} \mu & 0 \\ 0 & -(1-\mu) \end{bmatrix} \begin{bmatrix} d\pi_G \\ d\pi_B \end{bmatrix} = D^2 c(q^*) \begin{bmatrix} dq_G^* \\ dq_B^* \end{bmatrix}$$

We obtain $dq_s^*/d\pi_{s'}$ for $s, s' \in \{G, B\}$ by setting either $d\pi_B = 0$ or $d\pi_G = 0$ and solving for the derivatives of interest. Specifically, left-multiplying the above expression by $\left(D^2 c(q^*)\right)^{-1}$, setting $d\pi_B = 0$ and dividing by $d\pi_G$ yields

$$\left(D^2 c(q^*)\right)^{-1} \begin{bmatrix} \mu & 0 \\ 0 & -(1-\mu) \end{bmatrix} \begin{bmatrix} 1 \\ 0 \end{bmatrix} = \begin{bmatrix} dq_G^*/d\pi_G \\ dq_B^*/d\pi_G \end{bmatrix}$$

If alternatively, we set $d\pi_G = 0$ and divide by $d\pi_B$, we obtain

$$\left(D^2 c(q^*)\right)^{-1} \begin{bmatrix} \mu & 0 \\ 0 & -(1-\mu) \end{bmatrix} \begin{bmatrix} 0 \\ 1 \end{bmatrix} = \begin{bmatrix} dq_G^*/d\pi_B \\ dq_B^*/d\pi_B \end{bmatrix}$$

Stacking these sidewards, and substituting the explicit expression for $\left(D^2c(q^*)\right)^{-1}$, we thus obtain

$$\frac{dq^*}{d\pi} = \begin{bmatrix} \frac{dq_G^*}{d\pi_G} & \frac{dq_G^*}{d\pi_B} \\ \frac{dq_B^*}{d\pi_G} & \frac{dq_B^*}{d\pi_B} \end{bmatrix} = \frac{1}{\det\left(D^2c(q^*)\right)} \begin{bmatrix} \mu c_{22} & (1-\mu)c_{12} \\ -\mu c_{21} & -(1-\mu)c_{11} \end{bmatrix}$$

By strict convexity of c, $\det\left(D^2c(q^*)\right) > 0$. By Schwarz's theorem, $c_{12} = c_{21}$, which, by assumption, is positive. Hence, $\frac{dp_G}{dm} = \frac{dq_G}{dm} = \frac{\mu c_{22}+(1-\mu)c_{12}}{\det(D^2c(q^*))} > 0$, and $\frac{dp_B}{dm} = -\frac{dq_B}{dm} = \frac{\mu c_{21}+(1-\mu)c_{11}}{\det(D^2c(q^*))} > 0$, which directly implies the claim.

(ii) (This proof also applies to the continuous model. For the two-states case, write $\omega \in \Omega = \{G, B\}$, with $\mu_G = \mu$ and $\mu_B = 1-\mu$.)

The cost function c is *posterior separable* if there exists a strictly convex function $h : [0,1] \to \mathbb{R}$ such that c can be written as

$$c\big((p_\omega)_\omega\big) = pE\left[h(\gamma_a^\omega)\right] + (1-p)E\left[h(\gamma_r^\omega)\right] - E\left[h(\mu)\right]$$

where $\gamma_a^\omega = \frac{p_\omega \mu}{p}$ denotes the value of the posterior density at ω if the agent participates, and $\gamma_r^\omega = \frac{(1-p_\omega)\mu}{1-p}$ if he abstains, p_ω is the probability that he participates in state ω, and $p := E(p_\omega)$. Here, all expectations are taken with respect to the prior distribution.

Differentiating the cost function with respect to p_ω, we get

$$\begin{aligned} \frac{\partial c}{\partial p_\omega} &= \mu_\omega\left[h(\gamma_a^\omega) + h(\gamma_r^\omega)\right] + ph'\left(\gamma_a\right)\left(\frac{\mu_\omega}{p} - \frac{p_\omega \mu_\omega^2}{p^2}\right) \\ &\quad + (1-p)h'(\gamma_r^\omega)\left(\frac{-\mu_\omega}{1-p} + \frac{(1-p_\omega)\mu_\omega^2}{(1-p)^2}\right) \\ &= \mu_\omega\left[h(\gamma_a^\omega) + h(\gamma_r^\omega)\right] + h'\left(\gamma_a\right)\mu_\omega(1-\gamma_a^\omega) - h'\left(\gamma_r\right)\mu_\omega(1-\gamma_r^\omega) \end{aligned} \tag{6}$$

where $\gamma = (\gamma_a, \gamma_r)$ denotes the distribution of posterior beliefs. For $\omega' \neq \omega$, γ_a^ω and γ_r^ω depend on $p_{\omega'}$ only through p. Therefore, we have that $\frac{\partial^2 c}{\partial p_\omega \partial p_{\omega'}} = \frac{\partial^2 c}{\partial p_\omega \partial p}\frac{\partial p}{\partial p_{\omega'}} = \frac{\partial^2 c}{\partial p_\omega \partial p}\mu_{\omega'}$, so that it suffices to show that $\frac{\partial^2 c}{\partial p_\omega \partial p} < 0$. Indeed,

$$\frac{\partial^2 c}{\partial p_\omega \partial p} = \mu_\omega h''(\gamma_a^\omega)(1-\gamma_a^\omega)\frac{\partial \gamma_a^\omega}{\partial p} - \mu_\omega h''(\gamma_r^\omega)(1-\gamma_r^\omega)\frac{\partial \gamma_r^\omega}{\partial p}$$

Since $\frac{\partial \gamma_a^\omega}{\partial p} = -\frac{\gamma_a^\omega}{p}$ and $\frac{\partial \gamma_r^\omega}{\partial p} = \frac{\gamma_r^\omega}{1-p}$, by the definition of γ_a^ω and γ_r^ω we obtain

$$\frac{\partial^2 c}{\partial p_\omega \partial p} = -h''(\gamma_a^\omega)\gamma_a^\omega(1-\gamma_a^\omega) - h''(\gamma_r^\omega)\gamma_r^\omega(1-\gamma_r^\omega).$$

This is negative due to $h'' > 0$ and $\gamma_a^\omega, \gamma_r^\omega \in [0,1]$ for all $\omega \in \Omega$.

Proof of proposition 2 Consider the posterior separable cost function, $c(p_G, p_B) = ph(\gamma_a) + (1-p)h(\gamma_r) - h(\mu)$, for some strictly convex, twice differentiable function $h : [0,1] \to \mathbb{R}$. Due to posterior separability and due to the bijective relation between state contingent choice probabilities (p_G, p_B) and posterior beliefs (γ_a, γ_r), the agent's optimization problem can be recast as

$$\max_{\gamma_a, \gamma_r} pN_a + (1-p)N_r \quad \text{s.t.} \quad p\gamma_a + (1-p)\gamma_r = \mu,\ \gamma_a \geq \mu,\ \gamma_r \leq \mu \tag{7}$$

where

$$\begin{aligned} N_r &= 0 - \lambda h(\gamma_r) \\ N_a &= \gamma_a(\pi_G + m) + (1-\gamma_a)(\pi_B + m) - \lambda h(\gamma_a) \end{aligned}$$

and where the first constraint is the law of iterated expectations.

Caplin and Dean (2013b) refer to N_a and N_r are the *net utilities* of each act. They show that the solution to (7) is given by the posteriors γ_a and γ_r that support the concavification of the upper envelope of the net utility functions, akin to the well-known result by Gentzkow and Kamenica (2011).

To find γ_a and γ_r that support the concavification, we derive the tangent to N_r at γ_r and the tangent to N_a at the belief γ_a. First, insisting that the slopes of the tangents coincide yields

$$-\lambda h'(\gamma_r) \quad = \quad \Delta - \lambda h'(\gamma_a) \tag{8}$$

Second, we require that the levels of the tangents are equal at the level of belief $\tilde{\gamma}$ that renders the agent just indifferent between participating and abstaining (i.e. $\tilde{\gamma}$ satisfies $0 = \tilde{\gamma}\pi_G + (1-\tilde{\gamma})\pi_B + m$). Writing, for ease of notation, $\Delta = \pi_G - \pi_B$, we thus obtain:

$$-\lambda h(\gamma_r) \quad = \quad \Delta(\gamma_a - \tilde{\gamma}) - \lambda h(\gamma_a) + (\gamma_a - \gamma_r)\lambda h'(\gamma_r) \tag{9}$$

We now show that $\frac{\partial \gamma_r}{\partial m}$ has the same sign as $\frac{\partial \gamma_a}{\partial m}$. To do so, we differentiate (9) with respect to m. This yields $-\lambda h''(\gamma_r)\frac{\partial \gamma_r}{\partial m} = -\lambda h''(\gamma_a)\frac{\partial \gamma_a}{\partial m}$. Hence the statement follows from the fact that $h''(\gamma) > 0$ for all $\gamma \in [0,1]$. Taking the derivative of (9) with respect to m then shows that

$\frac{\partial \gamma_a}{\partial m} < 0$, as follows. The derivative is

$$-\lambda h'(\gamma_r)\frac{\partial \gamma_r}{\partial m} \quad = \quad \Delta\left(\frac{\partial \gamma_a}{\partial m} - \frac{\partial \tilde{\gamma}}{\partial m}\right) - \lambda h'(\gamma_a)\frac{\partial \gamma_a}{\partial m} + \left(\frac{\partial \gamma_a}{\partial m} - \frac{\partial \gamma_r}{\partial m}\right)\lambda h'(\gamma_r) + (\gamma_a - \gamma_r)\lambda h''(\gamma_r)\frac{\partial \gamma_r}{\partial m}$$

This simplifies to

$$0 = \frac{\partial \gamma_a}{\partial m}\big(\underbrace{\Delta - \lambda h'(\gamma_a) + \lambda h'(\gamma_r)}_{=0,\text{ by (9)}} + (\gamma_a - \gamma_r)\lambda h''(\gamma_a)\big) - \Delta\frac{\partial \tilde{\gamma}}{\partial m}$$

Since $\frac{\partial \tilde{\gamma}}{\partial m} < 0$ and $(\gamma_a - \gamma_r)\lambda h''(\gamma_a) > 0$, we thus have that $\frac{\partial \gamma_a}{\partial m} < 0$.

Proof of proposition 4 The incentive is $\bar{m}$ with probability α and $\underline{m}$ with probability $1-\alpha$, with $\bar{m} > \underline{m}$. Since $U(p_G, p_B; m)$ is linear in m, the agent's expected utility from state-contingent acceptance-probabilities (p_G, p_B) is $U(p_G, p_B; \alpha\bar{m} + (1-\alpha)\underline{m}) - \lambda c(p_G, 1-p_B)$. This has increasing differences in $(p_G, 1-p_B, \alpha)$ because $U(p_G, p_B; m) - \lambda c(p_G, 1-p_B)$ has increasing differences in $(p_G, 1-p_B, m)$. Consequentially, by Topkis' theorem, (p_G, p_B) is increasing in α. Hence, the fraction of participants, given by $p = \mu p_G + (1-\mu)p_B$, is therefore also increasing in α.

Proof of proposition 3

(i) I use Topkis' theorem to prove the claim. The agent maximizes expression (5) by choosing the family of state-contingent participation probabilities $(p_\omega)_\omega$. We must show that the objective function has increasing differences both in $(p_\omega, p_{\omega'})$ and in (p_ω, m) for all $\omega, \omega' \in \Omega$. As m enters the objective function additively, the latter is trivially true. Because the objective function is non-linear in $(p_\omega, p_{\omega'})$ only through the cost function c, it is sufficient to show that c has decreasing differences in $(p_\omega, p_{\omega'})$. This is shown in the proof to part (ii) of proposition 1 above.

(ii) A direct application of theorem 1 in Matějka and McKay (2015) shows that for all $\omega \in \Omega$, the state-contingent acceptance probabilities p_ω are given by

$$p_\omega \quad = \quad \left[1 + \left(\frac{1}{p} - 1\right)\exp\left\{-\frac{1}{\lambda}\omega\right\}\right]^{-1} \tag{10}$$

where p is the *ex ante* participation probability, $p = E(p_\omega)$.

I assume $P\big(\omega \in [-m'', -m]\big) = 0$ for some $m'' > m'$. (this condition is satisfied in the two-state model of section 4 by the assumption that $\pi_G + m' > \pi_G + m > 0 > \pi_B + m' > \pi_B + m$). The extension to the case in which $P\big(\omega \in [-m'', -m]\big)$ is sufficiently small follows by continuity. Moreover, without loss of generality, I set $m = 0$ (the prior distribution μ can be changed accordingly). We have $P(regret|participate) = P\big(\omega{+}m' \leq 0\,|participate\ \big) = \int_{-\infty}^{-m'} \frac{p_\omega}{p} d\mu(\omega)$. By equation (10) and the assumption that $P\big(\omega \in [-m'', -m]\big) = 0$, this is equivalent to

$$P(regret|participate) = \int_{-\infty}^{-m''} \frac{1}{p + (1-p)\exp\left[-\frac{\omega+m'}{\lambda}\right]} d\mu(\omega)$$

The denominator is decreasing in m' and decreasing in p (the latter is true because the integral is taken only over ω for which $\omega < -m'$, so that $\exp\left[-\frac{\omega+m'}{\lambda}\right] > 1$). By the proof of part (i) of this proposition, an increase in m' increases p_ω for all ω. Hence, it increases p. Consequently, a marginal increase in m' increases $P(regret|participate)$, as was to be shown.

C Experiment 2: Additional Analysis

C.1 Additional results

Result A1: Higher incentives do not change the deviation of elicited from Bayesian posteriors. Result 2 in section 5.2 has shown that higher incentives lead to more overoptimism. Why is that the case? There are two mechanisms through which this could happen. On the one hand, with higher incentives, subjects will rationally elect a different distribution of posterior beliefs. If the magnitude of the deviation of elicited from Bayesian posteriors varies with the Bayesian posterior, this will translate into varying overoptimism overall. (Graphically, this idea corresponds to a movement *along* the curve in figure 5.) On the other hand, it is conceivable that incentives alter how much subjective beliefs deviate from Bayesian beliefs, for any posterior. (This is represented by a *change* of the curve in figure 5.) To distinguish between these mechanisms, Columns 2 and 3 of Table C.15 display the relation between subjective and Bayesian posterior beliefs separately for the high and the low incentive condition. They are similarly close across these two conditions (Column 4 shows that a statistically significant difference at the 5% level occurs only for the elicited posterior 0.25), suggesting that incentives change the overall extent of overoptimism because they lead to a different distribution of Bayesian posteriors, not because they induce people to distort a given Bayesian posterior to a different extent.

Result A2: Elicited posteriors are not an *ex post* rationalization of the decision to bet. The results about subjective posterior beliefs would be difficult to interpret if elicited beliefs were simply an *ex post* rationalization of the decision to bet. To test whether this is the case, I again consider the difference between elicited and objective posteriors. If elicited beliefs were an *ex post* rationalization of the betting decision, then subjects who took the bet should appear more optimistic than those who refused it, *for any objective posterior*. That is, the graph in figure 6 should shift to the right if we consider only subjects who took the bet, and should shift to the left if we only consider those who refused it. By contrast, if beliefs inform the choice to bet, rather than *ex post* rationalize it, then objective and elicited beliefs should track each other similarly closely, regardless of whether subjects took the bet. Hence, I estimate Column 1 of Table C.16 separately for the cases in which subjects decided to bet, and for the cases in which they abstained; Columns 2 and 3 of Table C.16 display the results. In contrast to the *ex post* rationalization hypothesis, whenever the estimates differ at the 5%-level, it is the subjects who decided to reject the gamble who are more overly optimistic (Column 4). Hence, beliefs are *not* an *ex post* rationalization of the betting choice.

Variable	(1)	(2)	(3)	(4)
	Objective Posterior			*Difference*
Incentive	Both	High	Low	
Elicited Posterior				
0	0.023*	0.014	0.034	-0.02
	(0.013)	(0.014)	(0.024)	(0.028)
0.05	0.087	0.089	0.084	0.005
	(0.027)	(0.036)	(0.036)	(0.046)
0.15	0.161	0.137	0.180	-0.042
	(0.034)	(0.046)	(0.049)	(0.069)
0.25	0.316*	0.214	0.383**	-0.169***
	(0.036)	(0.047)	(0.046)	(0.061)
0.35	0.355	0.415	0.331	0.084
	(0.034)	(0.068)	(0.040)	(0.081)
0.45	0.441	0.498	0.413	0.084
	(0.034)	(0.059)	(0.042)	(0.073)
0.55	0.584	0.543	0.635*	-0.093
	(0.029)	(0.038)	(0.044)	(0.058)
0.65	0.677	0.671	0.687	-0.016
	(0.032)	(0.039)	(0.053)	(0.064)
0.75	0.668**	0.625**	0.750	-0.125*
	(0.035)	(0.046)	(0.057)	(0.074)
0.85	0.768*	0.773	0.760	0.014
	(0.042)	(0.051)	(0.064)	(0.076)
0.95	0.883**	0.906	0.853*	0.053
	(0.030)	(0.039)	(0.051)	(0.066)
1	0.937**	0.918**	0.955*	-0.037
	(0.028)	(0.041)	(0.025)	(0.037)
Observations	2,012	1,006	1,006	-
#Subj	503	503	503	-

Table C.15: All regressions pool across both information conditions (*incentive first* and *picture first*). Standard errors clustered by subject. Asterisks indicate the level of statistical significance by which objective and elicited posteriors differ. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

Variable	(1)	(2)	(3)	(4)
	Objective posterior			*Difference*
Decision	Both	Accept	Reject	
Elicited posterior				
0	0.023*	0.000	0.024*	-0.024*
	(0.013)	(0.000)	(0.014)	(0.014)
0.05	0.087	0.473	0.080	0.393
	(0.027)	(0.355)	(0.027)	(0.356)
0.15	0.161	0.285	0.154	0.131
	(0.034)	(0.146)	(0.034)	(0.151)
0.25	0.316*	0.282	0.319*	-0.037
	(0.036)	(0.108)	(0.038)	(0.115)
0.35	0.355	0.393	0.349	0.043
	(0.034)	(0.100)	(0.036)	(0.107)
0.45	0.441	0.531	0.420	0.111
	(0.034)	(0.080)	(0.039)	(0.092)
0.55	0.584	0.576	0.596	-0.02
	(0.029)	(0.038)	(0.047)	(0.061)
0.65	0.677	0.669	0.708	-0.039
	(0.032)	(0.037)	(0.064)	(0.074)
0.75	0.668**	0.699	0.507**	0.192**
	(0.035)	(0.038)	(0.085)	(0.093)
0.85	0.768*	0.805	0.472**	0.332**
	(0.042)	(0.040)	(0.140)	(0.141)
0.95	0.883**	0.922	0.591**	0.331**
	(0.030)	(0.027)	(0.143)	(0.146)
1	0.937**	0.959**	0.564	0.395
	(0.028)	(0.018)	(0.277)	(0.278)
Observations	2,012	930	1,082	-
#Subj	503	470	484	-

Table C.16: All regressions pool across both information conditions (*incentive first* and *picture first*). Standard errors clustered by subject. Asterisks indicate the level of statistical significance by which objective and elicited posteriors differ. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

C.2 Analysis of ancillary treatments

There were two ancillary treatments. In one, subjects decided whether to take a win \$3 / lose \$3 bet. In the other, they decided whether to take a win \$0.5 / lose \$0.5 bet. In both cases, they knew what their choice would be before they examined the picture. Table C.17 shows how often subjects choose to take the bet in each treatment, both averaged over states, and separated. Subjects take the gamble more often as stakes increase. This is because the false negative rate drops by a significant 7.02 percentage points (s.e. 2.92) whereas the false positive rate is not statistically significantly different (it increases by 3.47 percentage points (s.e. 2.72) as stakes increase). While the decrease in the false negative rate is consistent with the model in section 4, that model would also predict a decrease in the false positive rate as stakes increase. Risk aversion is as plausible countervailing factor that can explain the experimental results.

State	(1) Both	(2) Bad	(3) Good
Levels			
Treatment $(-3, +3)$	48.64	29.19	68.09
	(1.62)	(2.07)	(2.15)
Treatment $(-0.5, +0.5)$	43.37	25.72	61.03
	(1.61)	(1.99)	(2.26)
Treatment effect	5.26**	3.47	7.06**
	(2.23)	(2.72)	(2.92)
Number of observations	1906	969	937
Number of subjects	953	735	719

Table C.17: Unconditional and state-dependent participation probabilities in the ancillary treatments. Standard errors clustered by subjects. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Asterisks are suppressed for levels.

Moreover, Figure C.9 plots estimates of Bayesian posteriors against elicited posteriors for decisions in the ancillary treatments only, as Figure 5 had done for the main treatments. Again, elicited posteriors track Bayesian posteriors remarkably closely. The only statistically significant deviation is at the elicited posterior 0.25.

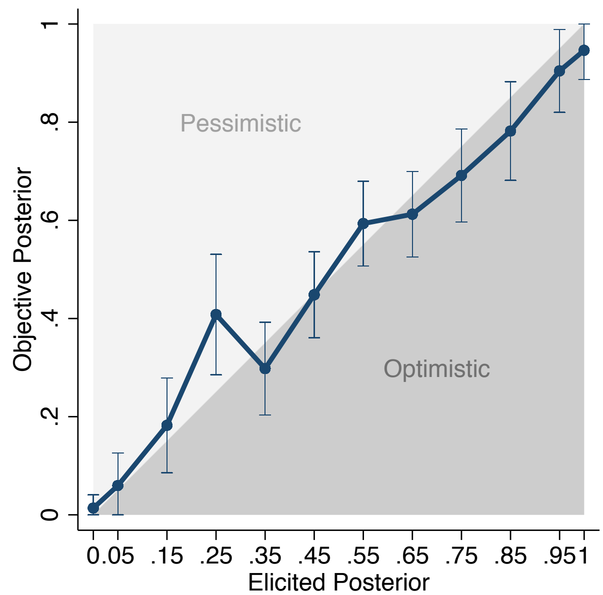


Figure C.9: Elicited and objective posteriors for ancillary treatments only. Whiskers indicate 95% confidence intervals. Standard errors clustered by subject.

D Experimental Materials

D.1 Video transcription and stimuli pictures

Figure D.10 displays photographs of the insects used for the experiment in section 3. The following is a transcription of the videos used in that experiment. The videos are available at `https://youtu.be/HiNnbYuuRcA` ("Why you may want to eat insects") and `https://youtu.be/ii4YSGOEcRY` ("Why you may not want to eat insects").

Transcription: Why You May Want to Eat Insects Five reasons you should consider eating insects. For your own personal health, and for the overall health of the planet, and, most importantly, for your pleasure, you should be eating more insects. This isn't meant as a provocative, theoretical idea. Here are five very serious reasons why you should consider increasing your insect intake.

First, insects can be yummy. You'd think that insects would have a pungent, unusual aroma. But they are actually very tasty, and considered a delicacy in many parts of the world. Also, like tofu, they often take on the flavor of whatever they're cooked with. That's why we are on the verge of a real insectivorous moment in consumer culture. The Brooklyn

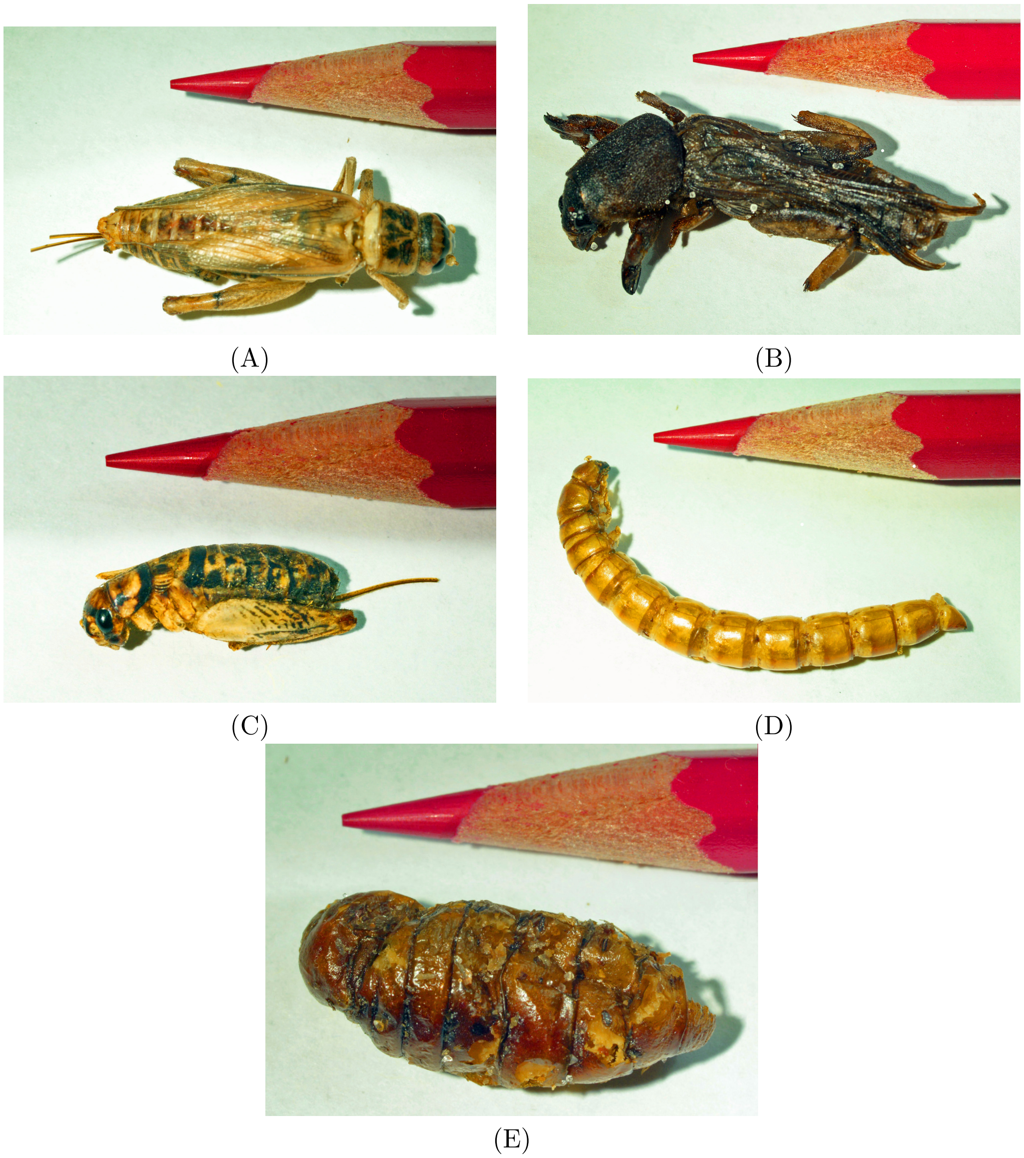


Figure D.10: Insects eaten by subjects. A. House cricket (acheta domesticus) B. Mole cricket (gryllotalpae) C. Field cricket (gryllus bimaculatus) D. Mealworm (tenebrio molitor) F. Silkworm pupa (bombyx mori).

startup Exo just started selling protein bars made from ground cricket flour, and the British company Ento sells sushi-like bento boxes with cricket-based foods. The restaurant Don Bugito in San Francisco's Mission district offers creative insect-based foods inspired by Mexican prehispanic and contemporary cuisine. "I am trying to bring a solution into the food market which is introducing edible insects" [Monica Martinez, owner of Don Bugito]. New cookbooks are entering the market, such as Daniella Martin's "Edible", or van Huis et al.'s "The insect cookbook". Don Bugito's reviews on yelp are glowing. Most Americans need some courage to take a bite. But once they do, they are pleasantly surprised. Morgane M., from Sunnyvale, CA describes her experience: "I saw their stand at the Ferry Building farmers market and decided to take the plunge. I tried the chili-lime crickets and they were surprisingly good! For the curious-but-apprehensive: the chili-lime crickets taste like flavorful, super crunchy (almost flaky) chips. That's it. If you've ever had super thin tortilla chips, you'll have an idea what to expect." Other people liked them even more. For example Nelson Q. from Las Vegas, NV: "This Pre-Hispanic Snackeria has made me a fan .... They had the most interesting menu items of the evening at Off The Grid ... Would I try insects again??? Yessir!...ALOHA!!! " Rodney H. from San Francisco agrees: "It's great! And the mealworms add kind of a nice, savory quality to it. You never would guess that you're eating an insect."

Second, insects are a highly nutritious protein source. "Insects are actually the most ... one of the most efficient proteins on the planet" [Monica Martinez]. It turns out that pound for pound, insects provide much higher levels of protein compared to conventional meats like beef, chicken, and fish. While eggs consist to just 12% proteins, and beef jerky clocks in at 33%, a single pound of cricket flour has 65% protein. That's twice as much as you get in beef jerky! Insects also have much higher levels of nutrients like calcium, iron, and zinc. They are also good sources of vitamin B12. That's an essential vitamin that's barely found in any plant-based foods and thus can be difficult for vegans to come by.

Third, our objection to eating insects is arbitrary. Your first reaction to this movie was probably a sense of dislike. But there's nothing innate about that reaction. For one, billions of people already eat insects in Asia, Africa, and Latin America every day. More generally, the animals considered to be fit for consumption vary widely from culture to culture for arbitrary reasons. Most Americans consider the idea of eating horses or dogs repugnant, even though there's nothing substantial that differentiates horses from cows. Meanwhile, in India, eating cows is taboo, while eating goat is common. These random variations are the results of cultural beliefs that crystallize over generations. But luckily, these arbitrary taboos can be defeated over time. There was a time when raw fish – served as sushi – was seen as repugnant in mainstream

US culture. Now it's ubiquitous. Soon, insects – which are closely related to shrimp – may be elegant hors d'oeuvres.

Fourth, insects are more sustainable than chicken, pork, or beef. “I think the biggest problem for United States right now is we eating to much cattle, too much meat” [Monica Martinez.] Insects are a serious solution to our increasingly pressing environmental problems. It takes 2000 gallons of water to produce a single pound of beef, and 800 gallons for one pound of pork. How much do you think is required for a pound of crickets? One single gallon! Producing a pound of beef also takes thirteen times more arable land than raising a pound of crickets. It needs twelve times as much feed, and produces 100 times as much greenhouse gases. These very handsome environmental benefits are why the UN has released a 200 page report on how eating insects could solve the world's hunger and environmental problems just two years ago. Needless to say, the UN strongly advocates for insects as a food source. And it's not just the UN. In 2011, the European Commission has offered a four million dollar prize to the group that comes up with the best idea for developing insects as a popular food.

Five, we already eat insects all the time. The majority of processed foods you buy have pieces of insect in them. The last jar of peanut butter you bought, for instance, may have had up to 50 insect fragments. A bar of chocolate can have about 60 fragments of various insect species. Some experts estimate that, in total, we eat about one or two pounds of insects each year with our food. These insects pose no health risks. The FDA does set limits, but they are simply set for aesthetic reasons in other words, so you don't actually see them mixed into your food.

To summarize, these are five very compelling reasons to give it a try. Five, we already eat insects all the time anyway. Four, insects are more sustainable and ethical than chicken, pork, or beef. Three, our objection to eating insects is completely arbitrary. Two, insects are a highly nutritious protein source. One. ”Most of people react really, really positive” [Monica Martinez]. Insects can be very tasty!

Transcription: Why You May Not Want to Eat Insects Four reasons you may want to avoid eating insects. Reason 1. Some cultures eat insects. But to those of us who are not used to it, insects can be... well, see for yourself. [American tourist in China] “Welcome to eating crazy foods around the world with Mike. And we're in China. If I've learned one thing about China it's they will eat absolutely everything. So you have caterpillars and you have butterflies. The pupae is what the caterpillar turns into before it turns into a butterfly. ... they don't look very appealing at all. But ... try everything once. So, up to the face. Hhh.” [Eats puppae.] “Not good. Ugh ... it ... it popped. It popped! It's just ... it's just too much for me." [Throws remaining pupae into trash bin.] [Bear Grylls] "Whoa! Ready for this? Oh my goodness! Pfh! This one has been living in there a very, very long time. I'm not gonna need to eat for a week after this. Pfh." [Eats live beetle larva.] "Argh! This actually ranks as one of the worst things I've ever, ever eaten!"

Reason 2. Insects have many body parts. Most of those parts we do not usually eat in other animals. Let's see those parts... [Biology student] "Let's take a closer look at some of the structures we see on this grasshopper. So the first thing I want to point out is that it has six legs. There are two pairs. Here is the pair of hindlegs. There's a pair in the middle here, on the middle segment of the thorax. Ok, those are the midlegs. And then there's another pair on the front here, those are the forelegs or prolegs. Ok? So there's six altogether, all insects have six legs, or three pairs of legs, it's characteristic of the class. Ok? So we also can see, right up here, there are a pair of wings. On each side of the body there are two wings. The forewing, k? – as in the one in front – and this is the hindwing down here, ok? So there are four wings on this animal. Other insects only have two, some have none. Now we'll move up to the head. The first thing you'll notice is this pair of long antennae. Ok, we've seen antennae in other animals. So, clearly, those are involved ... they have a sensory function. They're usually involved in a tactile, or a touch sensory function. Some of them are used in chemoreception, which would be like a smell or taste. And speaking of sensory organs, we got one more here, which we would be remiss to not mention, uhm, which is the large compound eye here. So, I've made an incision on the dorsal surface of this grasshopper. Ok? And I've peeled back the exoskeleton. And before I go digging too much, uhm, it's going to be difficult to see many structures, but on these individuals it's very easy to see, uhm, all of these very large and pronounced little sort of tubular looking structures. There's one right there. Those are all eggs."

Reason 3. When you eat an insect, you eat ALL of it. In particular, its digestive system, including its stomach, intestine, rectum, anus, and whatever partially digested food is still in there. [Biology student] "Now, if we move on to the digestive system... there is a mouth, of course, we talked about that being down here, ok? The mouth opens into a small pharynx, ok? And then it basically opens up into this large, dark, thin-walled sack right here, ok? This is the crop. Ok, so this is basically a food storage pouch right in here. So ... getting to the stomach, that's what we find next, this thin-walled, sort of darker colored sack right here, which I've just broken a little bit, that, uhm, is the stomach, all in here, ok? Below the stomach we find this slightly darker and a bit more muscular tube right here. That is the intestine. And the intestine opens into a short rectum and an anus."

Reason 4. Edible insects are perfectly save to eat. Nonetheless, we tend to associate insects with death and disease. Even if we know that eating some insects is harmless, this association is difficult to overcome. [Nature film maker] “Just a few days ago, one of those gaur was killed by a tiger in the night. This carcass is now probably about five days old, and, as you can see, absolutely riving with maggots of many different species.”

D.2 Instructions for the insect experiment

This section reproduces the instructions for the *video* treatment. The instructions for the no information treatment are identical, except that no video is mentioned.

Welcome

This is a study of individual decision-making and behavior. Money earned will be paid to you in cash at the end of this experiment.

YOUR DECISIONS IN THIS EXPERIMENT

Your decisions in this experiment determine your payment for this study, and what kind and amount of food products you will eat. There is *no deception* in this experiment: Whenever we tell you that something will happen if you choose a given option, then that thing *will* happen if you choose that option. Whenever we tell you that the computer will do something with a specified chance, then to computer *will* do that thing with exactly that chance. Anything else would violate the IRB protocol under which we run this study (IRB34001).

WHAT YOU MAY AND MAY NOT DO

On your desk you find a pen, paper, and a calculator. You may make use of any of them if you wish. You are not required to do so.

This is a study of **individual** decision-making, which means you are **not** allowed to talk during the study. If you have any questions, please raise your hand and we will come and answer your questions privately.

Please do not use cell phones or other electronic devices until after the study is over. Also, do not browse the internet, or check your emails. If we do find you doing one of these things, the rules of the study require us to deduct $20 from your payment. The only exception to this rule concerns the calculators that are provided on your desk.

Payment

This study has three parts. Parts 1 and 3 concern food items. Part 2 does not concern food items. Each part has several steps or rounds.

You will be paid at least $15 if you complete this experiment (this includes your showup payment of $5), regardless of your choices.

In addition, you receive payment for each part.

Part 1

1. You automatically receive $20 if you complete part 1 *and* follow through with the decisions you make in this part.
2. Depending on your choices and luck, you may win additional money.

Part 2

You may win or lose up to $10 from in this part. This depends both on your choices and on luck. Any earnings will be added to your payment from part 1. Any losses will be discounted from your payment in part 1.

Part 3

Depending on your choices and luck, you may earn additional money from this part.

At the end of this experiment, the computer will randomly select *exactly one* decision from part 1, *exactly one* decision from part 2, and *at most one* decision from part 3. Only these three decisions will determine your payment and consumption of food items in this study. Hence,

In each part, you should make each decision
as if it is the only one that counts—because it might be!

The payment for participation in this study will paid to you in cash at the end of this study.

Decision Lists

All parts of this experiment involve decision lists similar to the one below. What X is will vary across different decision lists.

Each line is a separate decision.

On each line, you are asked to select either the option on the right, or the option on the left.

If at the end of the study, the computer randomly chooses your payment from this study to be determined by a decision list, here's what will happen: The computer will randomly draw one line from that price list. Your payment will be determined according to the decision you made on that line. Your choices have *absolutely* <u>*no*</u> bearing on the line the computer may select.

Hence it is in your best interest to select on each line the option you genuinely prefer!

For instance, suppose that you filled in the decision list like this:

Suppose that the computer randomly selects the decision on the first line to be carried out. On this line you selected the option on the right. Therefore, you will not do X, and you will neither earn or nor lose any additional money.

Suppose instead that the computer randomly selects the third line for payment. On that line, you selected the option on the left. Therefore, you will do X, and $4 will be added to your earnings from this experiment.

Most people begin a decision list by choosing the option on the right and then switch to choosing the option on the left (as in the example above). For your convenience, once you click a given option, the computer automatically selects the option on the right on all lines above the line you clicked, and automatically selects the option on the left on all lines below the line you clicked.

Part 1 (involving food items)

Most of the decisions you will make in part 1 are about whether or not you want to consume some food item in exchange for a specified amount of money. Many of the food items may be unfamiliar and / or unappealing to you.

IMPORTANT:
*You will always be given the choice not to eat any food item at all! You will **never** be forced to eat any food item!*

All food items will be eaten in the small waiting room next to this room. At each time, there will be at most one participant in that room. Hence, except for the study staff, consumption of the food items occurs in private.

Recall: This study involves *no deception*. Whenever we tell you that something will happen if you choose a given option, then that thing *will* happen if you choose that option. In particular, if you decide to eat a food item in exchange for a specified amount of money (and that decision is selected to be carried out by the computer), then you will get that amount only if you completely eat the food item.

What happens if during the experiment, I decide to eat some food item for some price, and at the end of the experiment, when I should eat the item, I change my mind?

You are allowed to finish the study without eating the food item. In this case, because you did not follow through with your decisions, you will not receive the $20 you would have received had you instead completed part 1 *and* followed through with your decision.

Example

Suppose you are given the offer

"Eat [specified food item]. In exchange, receive $5.",

Suppose your decision on this offer is randomly selected to be carried out at the end of this study.

If you **accept** the offer:

- If, at the end of this study, you **follow through** with your decision and eat the food item, you will receive the $5 you were promised in the offer. In addition, you will receive $20 for following through with your decision. Hence, your **total** payment from part 1 will be **$25**.
- If, at the end of this study, you **decide not to eat** the food item, even though you have accepted the offer, then you will *not* receive the $5 you were promised in the offer. You will also forfeit the $20 you would have received for following through with your decision. Hence, your **total** payment from part 1 will be **$0**.

If you **reject** the offer:

- You will receive the $20 you are promised for following through with your decision. Because you rejected the offer, you will not eat a food item, and you will not receive any additional money for part 1. Hence, your **total** payment for part 1 will be **$20**.

Main Decisions

Some participants will be presented with offers to eat specified food items in exchange for $3. For instance, they will decide whether or not to accept offers such as this one:

Eat [specified food item]. In exchange, receive $3.

Other participants will be presented with offers to eat specified food items in exchange for $30. Those participants will decide whether or not to accept offers such as this one:

Eat [specified food item]. In exchange, receive $30

All participants will be offered *exactly the same* food items, and in the same amount, regardless of how much they are paid. The **only** difference is that some participants will be offered a higher amount of money for eating any given food item.

There are five different food items in this study. For each of the food items you will make a decision such as the above. You will either be offered $3 in exchange for eating the item in **all** these decisions, or you will be offered $30 in exchange for eating the item in **all** of these decisions.

There is an **80% chance** that at the end of this study, the computer selects one of these five main decisions to be carried out.

Part 1 consists of the following 8 steps.

Step 1: Learn how much money you will be offered in the main decisions

At the beginning of the experiment, you will click a button to make the computer randomly decide whether you will be offered $30 in all five main decisions, or whether you will be offered $3 in all five main decisions.

The computer will immediately tell you which one got selected.

Step 2: Video

The food items in this study may be unfamiliar to you. To help you with your decisions, you will choose videos to watch.

You will watch the video you selected *before* you make any decisions regarding food items.

Step 3: Decision Lists, round 1

You will fill in five decision lists similar to the one below, one for each of the five food items.

Eat [specified food item]. In exchange receive $0.	○	○	Do not participate in the transaction on the left
Eat [specified food item]. In exchange receive $2.	○	○	Do not participate in the transaction on the left
Eat [specified food item]. In exchange receive $4.	○	○	Do not participate in the transaction on the left
Eat [specified food item]. In exchange receive $6.	○	○	Do not participate in the transaction on the left
Eat [specified food item]. In exchange receive $8.	○	○	Do not participate in the transaction on the left
...	○	○	...

The food items will be described in words. No further information will be given.

There is a **7 percent** chance that at the end of this study, the computer selects one of the decisions you make in one of these five lists to be carried out

Step 4: Main decisions

It is at this stage that you will make the five main decisions that were described before. The food items will be described in words. No further information will be given. There is a **80% chance** that one of the decisions you make in this part will be carried out.

Step 5: Survey

You will answer some survey questions. Your answers do not affect your payment from this study.

Step 6: Food items handout

Each participant will be given five containers, each containing one of the five food items. You may closely inspect the food items.

Step 7: Decision Lists, round 2

You will fill in five decision lists as before, one for each of the five food items.

There is a **7 percent** chance that at the end of this study, the computer selects one of the decisions you make in one of these five lists to be carried out

Step 8: Additional decisions

You will make a number of additional decisions. You will learn what these are later. There is a **6 percent** chance that one of these decisions will be carried out for payment.

Summary of part 1

Let's recap. Here's how part 1 proceeds:

1. You learn how much money you will later be offered in exchange for eating food items (in step 4 below).

2. The food items in this study may be unfamiliar to you. To help you with your decisions, you may select choose videos to watch.

3. **Decision lists, round 1**. You will fill in five decision lists, one for each of five different food items. The food items will be described in words, but no further information will be given.

   *There is a **7% chance** that one of these decisions will be carried out.*

4. **Main decisions.** You will make five decisions whether or not to participate a transaction such as this:

   "Consume [specified food item]. In exchange, receive $[payment]".

   You will either be offered *payment* = $30 in all five decisions, or you will be offered *payment* = $3 for all five decisions. Again, the food items will be described in words, but no further information will be given.

   *There is an **80% chance** that one of these five decisions will be carried out.*

5. You will complete a short survey.

6. Food items will be handed out, and you will be able to inspect them as closely as you wish.

7. **Decision lists, round 2.** You will fill in five decision lists, one for each of five different food items.

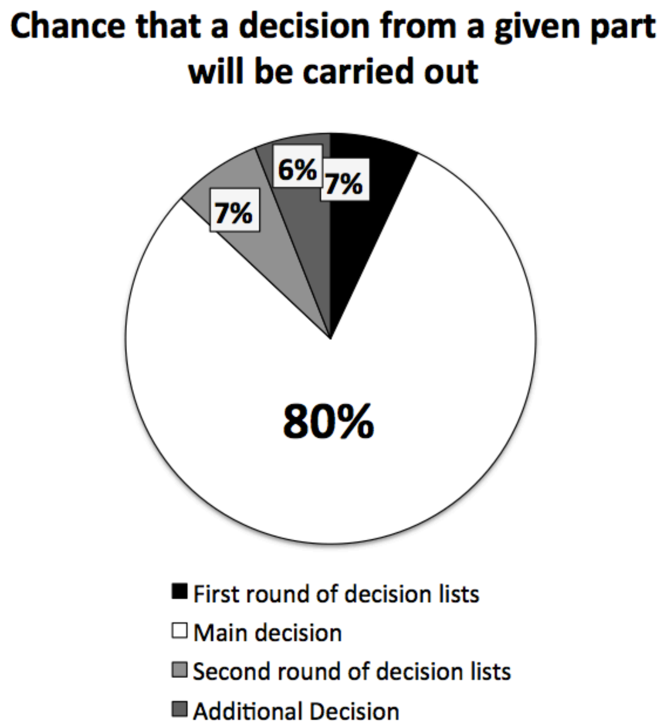
   *There is a **7% chance** that one of these decisions will be carried out.*

8. Additional decisions. These will be explained later.

   *There is a **6% chance** that one of these decisions will be carried out.*

At the very end of this study, the computer will randomly select exactly one of your decisions from this part to be carried out, according to the chances specified above.

This chart illustrates the chances that a decision from any given step of part 1 will be chosen to be carried out:



Parts 2 and 3

The instructions for parts 2 and 3 of this study will be displayed on your screen right before those parts begin.

You now have 5 minutes to read through these instructions on your own pace.

If you have any questions about this study, please raise your hand. We will come by and answer them privately.

Recall

This is a study of ***individual*** *decision making. Hence, you are not allowed to talk.*

Please do not use cell phones or other electronic devices until after the study is over. Also, do not browse the internet, or check your emails. If we do find you doing one of these things, the rules of the study require us to deduct $20 from your payment. The only exception to this rule concerns the calculators that are provided on your desk.

Additional decisions: Estimate what other participants did

48 participants have previously completed this study.
(These participants participated on May 8 and 9, 2015)

Your task is to estimate the least amount of money for which the average participant would eat a given food item.

To be very precise: Suppose we had offered each of the previous participants just enough so (s)he would eat the food item. How much money would we have spent, on average, per participant, for each food item?

Since all of the previous participants have completed the same decision lists as you, we know the correct answer to this question. We calculated the correct answer only using the choices participants had made BEFORE the food items had been handed out.

You will answer 15 questions in this stage.

Payment for this stage

There is a 6% chance that **one** of your decisions in this part will be carried out.

We told you in the beginning that you would receive $20 for following through with your decisions. If this stage of the experiment you do not make any decisions regarding food items. Instead, you will get these $20 if your estimate on the question that is selected for payment is perfect.

If your estimate is off, we will discount money from these $20. Specifically, for each $1 your estimate differs from the true amount, we will discount $0.50 from your payment.

For instance, if you underestimate the true average by $10, we will discount 10 * $0.50 = $5 from your payment, and you will earn $15 for part 1 of this experiment. If you overestimate the true average by $15, we will discount 15 * $0.50 = $7.50 from your payment, and you will earn $12.50 for part 1 of this experiment.

If you have any questions, please raise your hand.

The following five questions are the same as those before.

However:

This time we are asking you only about those participants who were offered **$30** in the main decisions.

That is, we are asking:

How much would the average participant **amongst those who were offered $30 in the main decision** at least need to be paid to eat [specified food item].

The following five questions are the same as those before.

However:

This time we are asking you only about those participants who were offered **$3** in the main decisions.

That is, we are asking:

How much would the average participant **amongst those who were offered $3 in the main decision** at least need to be paid to eat [specified food item].

D.3 Instructions for the experiment in the stylized setting

WELCOME

This is a research study run by the department of economics at Stanford University.

This survey will take about 40 MINUTES to complete.

In addition to your HIT payment, you will get a bonus of $3 for completing this study.

HOWEVER, depending on your choices, and on luck, you may get up to an additional $3 (so that you earn, a total of $9, including the payment for the HIT), or lose that bonus (so that you earn only the HIT payment of $3)

Please pay FULL ATTENTION to the study.

Please take this study only once. If you have participated in this, or a similar study before, the survey won't let you continue.

By clicking the button below, you consent to participating in this research study.

Instructions

PLEASE READ CAREFULLY

You will have correctly to answer questions about these instructions to continue.

This study has 6 parts. Each part has two rounds.

At the end of the study, the computer will randomly select one part of this study, and one round within this part. Your earnings will be determined by that ONE decision you made in this part, and and by luck.

The study ends with a short survey.

ABOUT LUCK

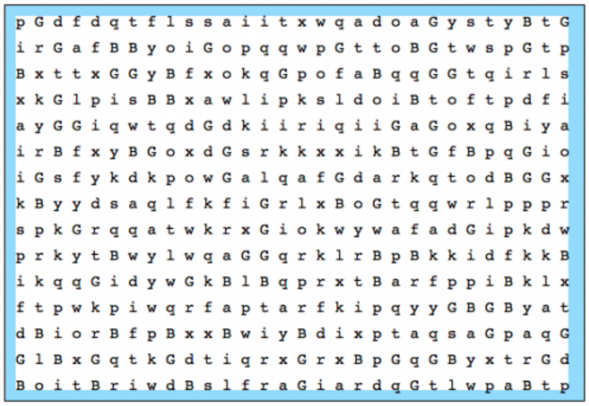
***We guarantee*, on behalf of Stanford University's Economics Department, that when we tell you that something will happen with some chance out of 100, it *will* happen with exactly that chance. Anything else would violate the IRB protocol under which we run this study (IRB26519).**

In this study, you will see several pictures of scrambled letters. Each worker will see different pictures.

Because only one part of the study will determine your entire payment, you should make each decision as if it is the one that counts - it might be!

(Sometimes, the continue button will appear only after a few seconds.)

In each part of the study there will be a NEW picture consisting of many letters such as this one.



A picture can be GOOD or BAD. A picture is Good if it contains more letters *G* than letters *B*. Otherwise, it is Bad.

In each part, the computer randomly decides whether the picture in that round is Good or Bad, with equal chance. This does NOT depend on anything that happened before.

In each part, you will be given the option to bet on the picture.

If you bet, and the picture is Good (has more *G*s than *B*s), you will win money (added to your Bonus).

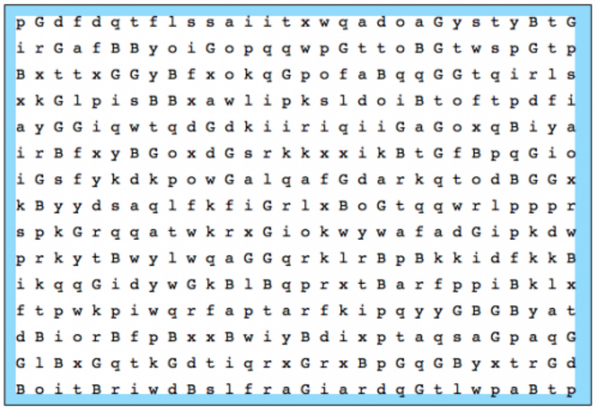
If you bet, and the picture is Bad (has more *B*s than *G*s), you will lose money (discounted from your Bonus).

A table like this will show you some information about the picture, and how much money you can win or lose.

	Good picture (chances 1 out of 2)	Bad picture (chances 1 out of 2)
Letters "G"	50	40
Letters "B"	40	50
If you bet on picture	***WIN $2***	***LOSE $2***

How much you can win and lose from betting is different in different parts.

Before you make your decision, you can get some idea whether the picture is Good or Bad. You will see the whole picture, as below; and can look at it for as long as you want.



IMPORTANT

Whether you will win or lose money from betting on the picture depends on whether there are more Gs than Bs IN TOTAL, and NOT on how many Gs and Bs you happen to have seen.

After you are finished examining the picture, you decide whether or not to bet on the picture.

- **If you take the bet:**
    - **if the picture is good, you win (added to your bonus)**
    - **if the picture is bad, you lose (discounted from your bonus)**
- **If you do not take the bet: You neither win nor lose any additional money.**

Sometimes you will not know how much you can win from betting on a good picture, or how much you can lose from betting on a bad picture when you examine the picture; you will only learn this just before you decide whether to bet. In that case, there's an equal chance that from betting on a good picture, you'll win \$0.50 or you'll win \$3, and an equal chance that from betting on a bad picture, you'll lose \$0.50 or you'll lose \$3

Each time after you have decided whether or not to bet on the picture the survey will ask you how certain you are that the picture you have just seen was a good picture, in a question such as the following:

How sure are you about the yellow picture?

It is...

definitely good	most likely good	very likely good	quite likely good	fairly likely good	slightly likely good	slightly likely bad	fairly likely bad	quite likely bad	very likely bad	most likely bad	definitely bad
100%	90-99%	80-90%	70-80%	60-70%	50 - 60%	40 - 50%	30-40%	20-30%	10-20%	1-10%	0%
○	○	○	○	○	○	○	○	○	○	○	○

The number below each option is the percentage chance with which you think the picture was good.

There is a 1 in 5 chance that your earnings from this study may be determined by your answer to such a question. Depending on your answer, the picture you have seen, and luck, your bonus may raise or fall by $0.50.

The payment procedure is designed such that it in your best interest to give your best, genuine answer to this question.

If you believe, for example, that it is about 75% likely that you have seen a good picture, it is in your best interest to select “quite likely good (70 - 80%)”. If you believe, for example that it is about 25% likely that you have seen a good picture (that is, you believe it is about 75% likely that you have seen a bad picture), then it is in your best interest to select “quite likely bad (20 - 30%).

One of your decisions from this study will be randomly selected for payment. Thus, you will be paid for EITHER for your bet on a picture (with a 4 in 5 chance), OR for the answer you give to the questions explained above, but never for both.

Read this if you would like to know more about the payment mechanism and about why it is in your best interest to answer this questions according to you true beliefs.
(We will *not* ask you test questions about the remaining material on this page.)

The payment procedure works like this.

For most choices you can select, there is a range of chances (for example 50 - 60%). Your payment is determined by the number in middle of the range you select (for example 55%, if you select the range 50 - 60 %).

Suppose you select a choice for which the middle of the range is some number X. The computer will randomly and secretly draw another number Y between 0 and 100. If the number the computer randomly draws is the larger one, that is if Y > X, then you will win $0.5 with chance Y in 100 (and lose $0.5 if you don't win). If the number you stated is the larger one, that is, if X > Y, then you will win if the picture you have seen is good. So if X is your genuine belief that the picture you have seen was good, you will win with chance X or with chance Y, whichever of the two is larger.

Why is it in my best interest to answer this question according to my genuine beliefs?

Simply, the reason is that you lower your chance of winning if you state a chance that is lower than you genuinely believe, and you also lower your chance of winning if you state something that is higher than you genuinely believe. So the best you can do is state what you genuinely believe.

To see why, it's best to go through an example.

Here's why you lose from stating a chance that is higher than you genuinely think is true. For example, suppose you genuinely believe the chance that the picture is good 60%, but you reply that you think it is good with a higher chance, say 90%. Suppose the number Y that the computer draws is between 60% and 90%, let's say it is 80%. That is lower than what you told us (which is 90%), so you will *not* play the computers' bet. Instead, you will win if the picture is good, which you genuinely think only occurs with 60% chance. The computers' bet would have given you a higher, 80%, chance instead. Hence, you hurt your chance of winning by stating the picture was more likely good than you genuinely think.

And here's why you lose from stating a lower chance than you genuinely think is true. For example, suppose again you genuinely believe the chance that the picture is good 60%, but you reply that you think it is good with a lower chance, say 10%. Suppose the number Y that the computer draws is between 10% and 60%, let's say it is 30%. That is higher than what you told us (which is 10%), so you will play the computers' bet and win with chance 30%. That is lower than if you had instead received the bet on the picture, which, according to your genuine belief, has a 60% chance. Hence, you hurt your chance of winning by stating the picture was less likely good than you genuinely think.

Therefore, the best you can possibly do is to select exactly the answer that corresponds to your genuine beliefs.

If you have any questions about this payment mechanism, please send an email to sambuehl@stanford.edu.

To make sure you got all of this, check all statements below that are true. You can only continue if you tick all boxes correctly.

Use the back button on the bottom if you would like to revisit the instructions.

(Do not try random combinations, there are far too many possible combinations. If you feel you understand the instructions, but still cannot continue, or otherwise have a question, send an email to sambuehl@stanford.edu)

- ☐ Whether I win or lose depends on whether there are more Gs than Bs in the picture IN TOTAL, and not on how many Gs and Bs I happen to have seen
- ☐ I can study the picture for as long as I like before I make a decision
- ☐ If I think there is a 60% chance I have seen a bad picture, this means that I think there is a 40% chance that I have seen a good picture
- ☐ It is possible that I think the chance that a picture was good is 75% and the chance that the same picture was bad is 50%
- ☐ If I am paid for a part with a picture, I will be paid EITHER for the bet I take on that picture, OR for the my answer to the question how certain I am about the picture I have seen, but NOT for BOTH
- ☐ When I am asked about how certain I am about a picture, I will earn most from this study if I state something a little higher than I truly think
- ☐ When I am asked about how certain I am about a picture, I will earn most from this study if I state exactly what I truly think
- ☐ At the end of the study, the computer will randomly select one decision I made. I will be paid for that and only that decision.
- ☐ Whether I win or lose depends on whether I HAVE SEEN more Gs than Bs, and not on how many Bs and Gs there are in the picture in total.
- ☐ In each part, I will see the same picture
- ☐ When I am asked about how certain I am about a picture, I will earn most from this study if I state something a little lower than I truly think

Stanford

Now the study starts.

Your decisions count for real money.

<< >>

	Good picture (chances 1 out of 2)	Bad picture (chances 1 out of 2)
Letters "G"	50	40
Letters "B"	40	50
If you bet on picture	*learn later*	*learn later*

Study the red picture for as long as you like, to learn whether you want to bet on the picture or not.

```
p y G B r f B r f f G G G f t t t G a B q l t B r B l t o r
y s y d r k B f x p d r o B y k l p w f y i i r x l l y w a
w i r k l f f B a B q q t q k G G p i a k x s k t q B B o w
f d G d y y k t k y s a t l w i p f l s i r y r a s q r o o
d B y k r y G f l G o p a G t x w a t o B y k B k y o r x B
s i f w B G y r q d i o y B w s s r o x o r o q t G o d t G
q B y y t l x i i x B s a r t f w y B B k G i G s p p i B x
o k r o B i a B r f s w a y w i B d o i G o a o G k r t G l
B y r w k i d a f w l y G a d f w B t t x B B a q G B G a G
B G k B d d p B t i B B i r r i s s s p G k B B B p G f y s
q r t q B B q k t p s y l d r B d G k G a x p G B l t f a y
B q r y a o s p i a f t G o a t r l y w s k q l l y y a r o
r t o k p s l f t x y B d B G o k r G y w o G k o B G y t G
i s o p d G s l G q f l B i B f t t k f B f s f i i x B i q
k a k i r k s s f q t o G a i i r q x i l a l p w p G w G p
```

Click NEXT to make your decisions.

(You can NOT go back once you click NEXT)

	Good picture (chances 1 out of 2)	Bad picture (chances 1 out of 2)
Letters "G"	50	40
Letters "B"	40	50
If you bet on picture	*WIN $3*	*LOSE $3*

Make a decision

- ○ Bet on the picture. WIN $3 if the red picture is Good, LOSE $3 if it is Bad
- ○ Do not bet on the red picture

How sure are you about the red picture?

It is...

definitely good	most likely good	very likely good	quite likely good	fairly likely good	slightly likely good	slightly likely bad	fairly likely bad	quite likely bad	very likely bad	most likely bad	definitely bad
100%	90-99%	80-90%	70-80%	60-70%	50 - 60%	40 - 50%	30-40%	20-30%	10-20%	1-10%	0%
○	○	○	○	○	○	○	○	○	○	○	○

(The number below each option is the percentage chance with which you think that the red picture is good.)

If you like, you may now go back to change your decision on whether or not to bet on the picture.